

AD 739552

ETL-CR-71-12

QUANTITATIVE GEOGRAPHY:  
ACHIEVEMENTS AND PROSPECTS

FINAL REPORT

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31 December, 1971

for

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Department of the Army  
Fort Belvoir, Virginia 22060

Project 4A061102B52D

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DAAK-02-70-C-0185

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## SUMMARY

The research goals and methodological approach of quantitative geography in its formative period (1955-1965) are described and some principles concerning the logic of scientific discovery which led to further adoption and modification of quantitative approaches in the discipline are discussed. Substantive achievements of quantification are described in relation to the analysis of spatial structure; mathematical regionalization; development of location theory from behavioral foundations; measurement problems in the analysis of spatial behavior; the development of spatial prediction models and in relation to quantitative spatial geomorphology. Forecasts of future developments in the area of quantitative geography are made and the adaptability of substantive findings for incorporation in an automated environment of data analysis are discussed.



## FOREWORD

Authorization for this study was from Contract DAAK02-70-C-0185, "Theory of Quantitative Geography" for the U.S. Army Engineer Topographic Laboratories Research Institute, Alexandria, Virginia. The objectives of the study were:

- "To review, define and clarify the current quantitative approach to geography. The study is to perform the following:
- a. Determine the validity, accuracy, and/or confidence of quantification as a geographic research tool.
  - b. Investigate quantitative geography's origin, review its antecedents, outline the periods of development and prepare a bibliography.
  - c. Delineate the methodology and clarify the terminology by showing the applicability or adaptability to geographic research.
  - d. Show how quantitative geography is relevant to terrain analyses.
  - e. Demonstrate quantitative geography's adaptability in the automated data processing environment."

During all but the final stages of preparation of this report, Dr. Roger A. Leestma was the contracting agency's officer in charge of the project and we would like to acknowledge our debt to him for first conceiving of the work, for conveying to us his enthusiasm for it and for arranging for a most positive critical review of an early draft of the manuscript.

We also thank both Dr. Leestma and his successor on the project, Mr. Nathan Fishel, for their understanding in extending the time requirements for preparing the manuscript so that we might critique and review our work. We cannot help but reflect that this report would have been easier to write a short three or four years ago. In the meantime and coinciding closely

with the period (since October 1969) in which we have worked in this area the quantitative thrust in geography has been increasingly questioned from within. In contrast with earlier decades when such questioning would have been of the value of quantification, per se; the present questioning is rooted in some fundamental epistemological issues and its resolution will influence the path that future quantitative studies will take. The almost single-minded purpose of early quantitative studies and the absence of differences in opinion on major issues that were characteristic of earlier times, are no more. By preparing a report which covers basic principles and common issues in the early chapters and which evaluates progress in substantive areas that have been cultivated by quite separate groups of researchers, our hope is that the variety of opinions within the quantitative school of geography will appear in the report. Though each chapter had its primary author with full freedom to discuss the selected area as he saw fit, our many collective discussions around the points made, (particularly in the early chapters), have led us recently to view the entire report as a collective endeavor.

At an early stage of the work we had the advantage of discussions with Dr. Leslie J. King, McMaster University, on the original project design. Through various drafts of most chapters our graduate students at the University of Iowa made many critical and valuable comments. We would particularly thank those who participated in the work, Mr. F. Ermuth, Mr. J. Hultquist, Mr. J. Louviere and Mr. R. Hall. Finally, we would like to thank our two secretaries, Ms. Anthoa Craven and Ms. Susan Moore who with much

patience, forbearance and good humor, struggled with our untidy manuscripts with the pressure of deadlines ever present.

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## INTRODUCTION

This report reviews the origins of the "quantitative revolution" in geography and assesses the degree to which fruitful findings have resulted from this trend toward increasing quantification of research in the discipline. Critical assessments of developments in any discipline that are oriented to only one facet of its methodology lose perspective and meaning unless they are related to other facets of methodology. Thus, at an early stage in the report we emphasize the relationship between theory development and quantification that many early quantifiers saw. Many of the substantive conclusions that later transpired could as much be laid at the door of theory construction as at that of quantification per se. Whether such developments should be attributed to quantification is a moot point - and not one that could be fruitfully debated. Suffice to note that the quantitative generalizations of many of the early empirical studies were seen as the primal "laws" on which theory would ultimately be built. The epistemological debates that followed led to further rationalizations concerning the character of a truly scientific geography and invariably these were couched in a form that called for further quantitative work. Thus did the goals of the discipline and those of quantitative geography reach a closer conformity until--in the minds of some--if a problem was not amenable to quantitative analysis it was often ruled outside the domain of the discipline! Viewed in this way, it seemed as interesting to enquire into how the new goals of geography became formulated as to enquire into whether the conventional goals of geographical research were furthered by quantification. Insofar as the new goals of

geography were customized to the character of the new quantitative geography, alternative (non-quantitative) approaches were no match for them.

The report covers three periods of geographical research. The emergence of quantification as a serious research-style is traced in Chapter II where the early quantitative literature of the modern era is systematically described in relation to broader principles of how disciplines become transformed. Reference is made there to many of the popular misconceptions concerning quantification that existed in the discipline. These are systematically examined in Chapter I in relation to general principles of mathematical logic and to the manner in which mathematics was generally viewed within the discipline at that time. That Chapter also discusses the types of mathematical models that may be constructed in geography. In Chapter III the logic apparently used in quantitative geography is contrasted with the reconstructed logic of paradigmatic areas of science in the logical positivist sense. Issues in theory development are discussed as is also the problem of choosing non-trivial axioms on which to develop deductive systems.

The second and third periods covered relate to the current status of quantification and future anticipated developments. These periods are not exclusively covered in separate chapters although Chapters IV and V are concerned predominantly with conventional modes of quantitative geographical research. In particular, Chapter IV reviews work in the quantitative description of spatial structure that comprised the largest proportion of quantitative geographical research in the first decade of the modern quantitative era (1955 - 1965). Chapter V discusses trends in regionalization and

classification in geography while Chapter VI and Chapter VII review two mainstreams of quantitative research both of which have received a great deal more attention in recent years. Thus our efforts have been directed rather deliberately to critically reviewing research areas that are prominent in contemporary quantitative geography. Chapter VI focuses on developments in location theory that relate to the more formal incorporation of the behavioral foundations of classical models and to the construction of computerized location models that have the facility for solving for locational patterns in areas of environmental variability--in contrast with the classical location models which were mathematically intractable for all but rather simple initial conditions. That chapter also discusses some of the measurement problems involved in deriving non-naive behavioral postulates for incorporation in location models. In particular, recent developments in non-metric, multi-dimensional scaling are reviewed and the widespread adaptability of this technique for locational problems is discussed. Chapter VII focuses on spatial forecasting and control models and after describing their salient characteristics, shows how many models are currently being designed to explicitly investigate policy issues which decision-makers and their agencies are interested in. In Chapter VIII a case study of Quantitative Spatial Research in Geomorphology serves both to provide documentation for points made in Chapters I through V and to assess substantive findings in the area of terrain analysis. Chapter IX surveys broadly the impact of quantification in the discipline and makes some predictions on the course of future work in quantitative geography.



## CHAPTER I

### ON THE USE OF MATHEMATICS IN GEOGRAPHY

In this chapter, we are concerned with how mathematics has been used in geography in relation to a number of general principles on the function of mathematical logic in scientific enquiry and in relation to the state in which geography presently finds itself. We concur with Coleman<sup>1</sup> that mathematics is a set of tools which must be fashioned in relation to the problems of a discipline and that the way in which it is used at any time will reflect the nature of these problems. Although we are primarily concerned with the principles that guide the application of mathematics to geography, we find it necessary to discuss contemporary applications with the context of the nature of contemporary problems in the discipline and to analyze some of the more frequently voiced objections to the use of mathematics in the solutions of such problems.

#### Objections to the Use of Mathematics in Geography

##### Some Common Misconceptions

A first objection pertains primarily to human geography and rests upon the belief that the attempt to extend the application of mathematics into the social sciences is doomed to failure because of the fundamental difference in the subject matter of the physical and social sciences. While conceding that the entities of, say, the science of mechanics are amenable to mathematical law, the geographer subscribing to this view would argue that human

beings are not.

The problems of geography, it would be argued, are distinguished from the physical sciences in that they are concerned with the decisions of man. Thus, many of the methods of the physical sciences, for example, the laboratory experiment, have little application to the social sciences. Some people have reached the conclusion that laws, in principle, cannot be found since such laws, if they did exist, would interfere with man's freedom to act. Thus, in geography, no less than in economics, sociology or political science, there have been dominant intellectuals who have taken their stand, somewhat as a matter of faith, that no laws or theories can be evolved with respect to man since the obvious freedom with which man chooses between alternative ends as well as alternative means to reach ends, is inconsistent with the notion that laws can exist to govern man's actions. This stand, as Kemeny points out,<sup>2</sup> is founded on the misconception that nature obeys laws as an obedient child is expected to conform to parental laws. In science, however, laws are descriptions of reality, so that whether or not such descriptions can be made of human actions is surely a question of fact -- not of faith. If we never search for such descriptions they will surely never be found. To claim that the search for laws will be fruitless is merely to express an opinion as to the outcome of the search. Before we conclude that it is harder to find laws in the social than the physical sciences, we should remember that the effort which social scientists

have devoted to this end is a very small fraction of that which physical scientists have expended in the last three centuries.

Do laws limit man's freedom to act? No! Of course man has freedom to make choices among available acts. But this freedom is tempered with reason such that his choice is aimed at securing a preferred combination of experience. Keirstead quotes Locke as saying,

"If to break loose from the conduct of reason, and to want that restraint of examination and judgement which keeps us from doing or choosing the worse, be liberty,<sup>3</sup> true liberty, madmen and fools are the only free men."

Thus, free men act to achieve certain goals and their essential freedom is the inner freedom to decide for themselves just what the outcome of the alternative acts might be. Laws that predict such outcomes on the basis of analysis of prior behavior do not make an automaton of man for two reasons. First, they can always be falsified by facts and, second, they may be couched in probability terms<sup>4</sup> and thus leave a variety of actions to be expected with different degrees of likelihood for any individual.

A second objection stems from the view that many phenomena and concepts in geography either cannot be "quantified" or cannot without a loss of "meaning". As Spate puts it,

"...there are matters on which precise quantified statements may be far more meager and unsatisfactory, and far less meaningful, than statements which do not rest on statistical enquiry but of which the accuracy cannot be impeached."<sup>5</sup>

Exactly what Spate means by "more meager", "unsatisfactory", and "meaningful" is not entirely clear, although from the examples he gives, it appears that he means that they are self-explanatory. For example, in referring to

Madrid and Barcelona, he states:

Analyses, useful so far as they go, can be made from the occupational structure of these two cities, analyses which are satisfactorily precise and valuable for a quantitative description. They are less meaningful--that is, they stand in more need of explanation and interpretation, of outside support--than conclusions drawn from the historical development of these two cities."<sup>6</sup>

Although one could attack the philosophy of explanation implicitly embraced by Spate, suffice to note that no supposed empirical fact provides its own explanation and that mathematics is not synonymous with precision in a measurement sense; if it were, group theory, graph theory, topology, etc., would have to be denied as legitimate branches of modern algebra and geometry. Since mathematics is comprised of languages or deductive calculi, only some of which are concerned with numbers and space, any declarative sentence, quantitative or qualitative, can potentially be expressed in a suitable mathematical form. To hold the belief that for all or for many of the problems in geography, numerical measurements are absent or irrelevant is not a sufficient basis for concluding that all mathematics is irrelevant to or inapplicable to these problems. Examples of the feasibility and usefulness of mathematizing qualities without first quantifying them in the social sciences have been well documented by Kemeny,<sup>7</sup> and Coleman.<sup>8</sup>

A third objection to the use of mathematics in geography and that which has been the most resistant to removal in the social sciences in general is the view that any mathematical system designed as a model of reality is, of necessity, oversimplified and does not represent all of the complexities

in that reality. Broek,<sup>9</sup> for example, in criticizing the emphasis on spatial organization in the BASS report<sup>10</sup> wrote that the report<sup>11</sup>

"... also reflects the fashionable pseudoscientific notion that only general principles are worth knowing or, as someone expressed it elsewhere, that extraneous noises must be eliminated so as to hear only the pure vibrations of theory. But is this not a matter of taste? What is noise to some people is music to others".

Arrow attributes a large part of the resiliency of this objection to the use of mathematics in social science to the fact that any natural language (e.g., English)

...is itself a social phenomenon and the multiple meanings of its symbols are very likely to be much better adapted to the conveying of social concepts than to those of the inanimate world. Furthermore, the empirical experience on which one's understanding of the social world is based consists to a large extent of symbolic expressions of other individuals; one can apprehend these expressions directly because one is himself part of the social world he observes. Such apprehension must inevitably take place on a largely unconscious level unamenable to mathematical expression (which is surely the acme of consciousness).<sup>12</sup>

Arrow goes on to suggest that of the social sciences economics is the field in which the use of mathematical methods has been most widespread and successful; this because the individuals studied by economists are engaged in relatively highly conscious calculating operations. Arguments such as this have been presented many times, particularly by Max Weber<sup>13</sup> and more recently by Winch<sup>14</sup> in attempts to demonstrate that social phenomena are not amenable to scientific investigation except through a methodology

based on Verstehen (empathic understanding). \* Closely connected with this view is one which denies the possibility of "objectivity" in social science. A refutation of these two viewpoints on logical grounds is presented by Rudner<sup>16</sup> and need not be presented here. However, to deny the necessity of Verstehen and the impossibility of objectivity in social science is not to deny that social phenomena are often exceedingly complex and that mathematical models of them are bound to be simplifications. This latter premise must be accepted. Even if it is granted that a mathematical model need only reproduce the "relevant features" of the real process the problem of complexity remains formidable. However, we argue that there is no alternative to simplification in the early stages of scientific investigation.

As Bartholomew states:

"The basic limiting factor is not the mathematical apparatus available but the ability of the human mind to grasp a complex situation. There is no point in building models whose ramifications are beyond our comprehension. Perhaps the only safeguard against over-simplification is to use a battery of models instead of a single one. Any particular model will be a special case of the more complex model which would be needed to achieve complete realism. Greater confidence can be placed in conclusions which are common to several special cases than in those applicable to one arbitrarily selected model."<sup>17</sup>

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\*That a version of Verstehen was still alive in geography as late as 1960 is demonstrated by Spate when he writes: "In the last resort we can only grasp the essence of regions, or a region, by a geographical empathy akin to the historical empathy demanded in Croce's famous passage about the Ligurian peasant..."<sup>15</sup>

While we grant that the complexity of the so-called "real world" is indeed overwhelming, no serious scientist (physical or social) has yet attempted or is ever likely to attempt the construction of an all-encompassing theory of it; instead the problems he sets for himself are relatively narrow and simple.<sup>18</sup>

The misunderstanding inherent in the argument that mathematics is not useful in fields concerned with explaining and predicting supposedly complex phenomena is seen then to be based on the presupposition that mathematics is of necessity over-simplified or at least simple. The rejection of mathematical languages in favor of natural languages for handling the problem of complexity implies that one of the latter languages is somehow more suitable for solving it. If such is the case it has yet to be proven and such a proof is unlikely to be in the offing since formal proofs are dependent upon the knowledge and acceptance of an artificial language. Kaplan cuts to the heart of the matter when he presents the famous anecdote:

"Prove to me," Epictetus was challenged, "that I should study logic." "How will you know that it is a good proof?" was the reply."<sup>19</sup>

Some more fundamental problems.

The objections to the use of mathematics in geography discussed above are clearly the result of misunderstandings as to the nature of mathematics. But there are other reasons why the use of mathematics in geography has not been more widespread. These stem from the stage of development most of the social sciences including geography find themselves:

1. Few generally useful and easily measurable sets of terms or

concepts have been posited in the non-physical sciences.

Perhaps the best example of an exception is, again, economics whose formal theories, with their set of concepts and specified interrelations, have found mathematics extremely useful.

Given this, it is not surprising that economic geography with its heavy reliance upon economic theory is that branch of geography in which the "quantitative revolution" began (see Chapter II).

2. A tendency (at least in the past) for non-physical scientists to avoid training in mathematics beyond very basic levels. This has contributed to the misunderstandings of mathematics discussed above.
3. The adherence to methodological positions at variance with those of the so-called scientific method. Harvey, for example, attributes the relative lack of explicit theory and laws in geography in part "to the methodological position of many geographers (particularly Hartshorne) which rested on the false inference that because we are essentially concerned with particular cases we must necessarily seek for only particular explanations."<sup>20</sup>
4. Both the findings of empirical research and the "theories" being tested have generally been so vaguely set forth that it is difficult to translate them into a mathematical language, and



"once translated they often fail to show an isomorphism with powerful parts of mathematics." <sup>21</sup>

We have specified two types of objections to the use of mathematics in geography and the social sciences: (1) those that categorically deny the relevance of mathematics due to inherent discontinuities between the subject matters of the physical and social sciences and (2) those that do not totally deny the usefulness of mathematics but point out some fundamental problems in social science and with social scientists which impede the adoption of mathematical model building. The latter type are the more serious and must be considered in more detail when we examine how mathematics has been used in geography. First, however, since the former set of objections were largely the result of misunderstandings we present a short section stating the modern viewpoint on the nature of mathematics.

#### The Nature of Mathematics

The purpose of this section is to show how mathematics can profitably be interpreted as abstract languages in contrast to more traditional views of the subject. As Kemeny puts it:

"A hundred years ago a mathematician would have defined mathematics as "the study of number and space." Indeed, the Thorndike - Barnhart Dictionary published in 1956 still defines mathematics as the "science dealing with numbers and the measurement, properties, and the relationships of quantities." The study of numbers led to the development of algebra, and the study of space to geometry. These disciplines merged in the calculus, the crowning glory of classical mathematics. A significant feature of modern mathematics is that such a definition is much too narrow to include its newer branches." <sup>22</sup>

The utility of mathematics does not lie in any notion of the "absolute truth" or necessary empirical truth of mathematical conclusions. The view that mathematics is a body of truths is the classical Greek opinion and that of mathematics as the most general empirical science was that of a dominant nineteenth century school of philosophy.<sup>23</sup> Both of these have been replaced by the "new mathematics." Mathematics is a set of abstract languages and mathematical reasoning is essentially deductive reasoning - the process of arriving at valid conclusions from accepted premises. Therefore any mathematical language is a deductive system with a distinctive structure. This view of the new mathematics has recently been stressed in the geographical literature by Harvey.<sup>24</sup>

Abstract languages.

A mathematical language begins with primitive or undefined terms including elements such as "point", "line", "number", etc. and relations such as "on", "between", "and", etc. Some terms in a language (any language) must remain undefined otherwise all definition would be circular. Having decided which terms to leave undefined, all other technical terms in a deductive system can be derived from these primitive terms with the help, of course, of ordinary non-technical words in a natural language. These derived terms are then said to be "well defined." From these terms an axiomatic system is developed containing axioms (variously called postulates or assumptions) and theorems. The former are sentences containing one or more undefined terms which are regarded as true. In Schaaf's words, the acceptability of an axiom "has nothing whatever to do with absolute truth,

nor with being self-evident, nor with empirical facts nor with intuition, observation, or common sense. (In mathematics) A postulate is simply an initial proposition which is accepted by all concerned and is designated as true."<sup>25</sup> The primary requisite of a usable set of postulates or axioms in a mathematical language is that they be consistent - namely that none in a set can be proved both true and false. Briefly then, a mathematical language always consists of (1) a few undefined terms, (2) words (symbols) defined in terms of these primitive terms, (3) some few initial declarative statements (propositions) called postulates, axioms or assumptions which are simply considered true by fiat and (4) many additional declarative statements called theorems, which are proved on the basis of the postulates by applying the conventional laws of logic (or a logic). At its simplest, mathematics consists of the form: if this set of postulates is true, then this set of theorems is true. It is clear then that the interpretation of mathematics as a set of artificial languages is "a far cry from the classical Greek idea of mathematics as a body of truths having a separate existence apart from our own minds."<sup>26</sup>

A miniature geometry.

Given the above characteristics of a mathematical language it can be appreciated that there are, potentially at least, an infinite number of such abstract languages all of which would enjoy the distinct advantages of clarity and consistency over the natural languages which are rich in

ambiguity. As an example consider the following miniature geometry adapted from Schaaf.<sup>27</sup> The undefined elements are the terms "point" and "line" and the undefined relation is "on" (where "point on line" and "line on point" are to be understood as having the same meaning). The following axioms are then set down:

- A<sub>1</sub> Each pair of lines is on at least one point.
- A<sub>2</sub> Each pair of lines is on not more than one point.
- A<sub>3</sub> Each point is on at least two lines.
- A<sub>4</sub> Each point is on not more than two lines.
- A<sub>5</sub> The total number of lines is 4.

It is readily seen that the set is consistent. Furthermore, a number of theorems can easily be proved from these axioms; for example:

- T<sub>1</sub> Not all lines pass through the same point.
- T<sub>2</sub> Any two distinct lines have exactly one point in common.
- T<sub>3</sub> Exactly two lines pass through (are on) each point.
- T<sub>4</sub> Every two lines passes through (are on) exactly three points.
- T<sub>5</sub> There are exactly six points.

T<sub>5</sub>, for example, follows directly from the joint occurrence of A<sub>3</sub>, A<sub>4</sub> and A<sub>5</sub> and is consistent with the familiar combinatorial formula  ${}_4C_2$ . i.e., the number of combinations of 4 objects taken 2 at a time is  $\frac{4!}{2!(4-2)!} = 6$ .

This miniature geometry can be given an interpretation as an empirical model as for example in the geometric configuration in Figure 1. For this configuration it is easily seen that the set of axioms and theorems are true.

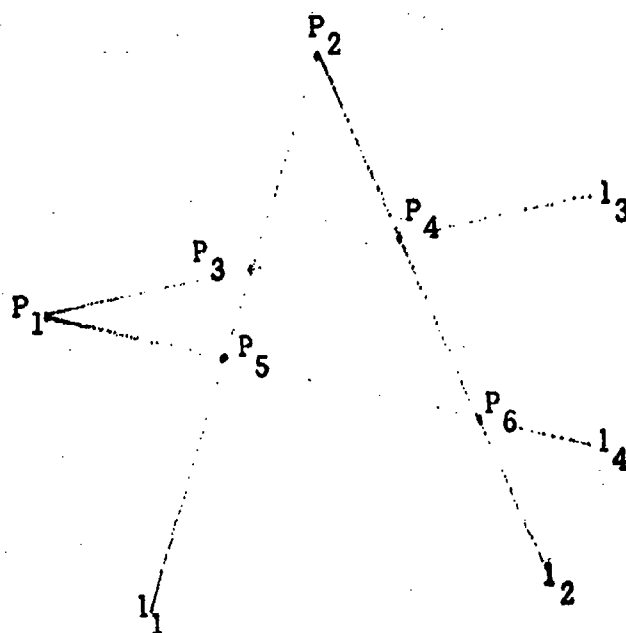


Figure 1. A Concrete Model of the Miniature Geometry

However, this geometric interpretation is by no means the only possible concrete model of this axiomatic system. By assigning appropriate meanings to the undefined elements and undefined relation, it is possible to construct other models by using in turn: (1) lines and planes; (2) musical notes and chords; people and committees; cities and airlines; etc. For example, we might let the "points" represent airports and the "lines" represent the routes of separate airlines.

The axioms in the model would be interpreted as follows:

- A<sub>1</sub> Each pair of airline routes shall have at least one airport in common.
- A<sub>2</sub> Each pair of airline routes shall have not more than one airport in common.
- A<sub>3</sub> Each airport shall be served by at least two airlines.
- A<sub>4</sub> Each airport shall be served by not more than two airlines.
- A<sub>5</sub> There shall be four airlines (and airline routes).

If for example the FCC regulated an airline system such that these and only

these assumptions were met, we could deduce that for this system:

- $T_1$  Not all airlines would stop (serve) at the same airport.
- $T_2$  Any two airlines serve (stop at) exactly one airport in common.
- $T_3$  Each airport is served by exactly two airlines.
- $T_4$  Each airline serves exactly three airports.
- $T_5$  There are only six airports in the system.

Admittedly, this example is extremely simple and the theorems follow straightforwardly from the axioms. The point to emphasize is that from the five axioms considerably more information about the airline system can be deduced some of which may be surprising; and this must be true if we grant the truth of the axioms. This example also relates back to the notion of isomorphism alluded to earlier. Since the structure of the airline system is the same as that of the "miniature geometry" they are isomorphic and any theorems arrived at in the abstract system have their equivalents in the airline system. Hence, whenever we can establish an isomorphism between a concrete or "real world" problem and a mathematical language we are in a most fortunate position. We can then utilize the mathematical language to unambiguously model those aspects of the "real world" of interest to us. Most importantly, we can arrive at conclusions many of which would have eluded us had we continued to work in one of the natural languages. In short, mathematics provides a set of deductive calculi into which empirical science can map some of its problems and theories and thereby arrive at logically sound solutions and conclusions. Such is the aim of all science. Science is, after all, problem solving!

### How Mathematics has been used in Geography

This section is divided into two parts. Directly following on from the last section we describe examples of the classical, or ideal, situation where we have been able to map a geographical problem into a mathematical language. Secondly, however, we recognize that quite often a mathematical language has not existed that is suitable for a geographic problem. This second situation is often ignored in statements concerning the use of mathematics in empirical sciences although the geographic experience would seem to suggest that approaches developed to overcome this deficiency make up a sizeable portion of "quantitative" research. Thus in the first part of this section we treat the case where languages are available and in the second we deal with quantitative approaches used where languages are not available.

#### Languages available: the isomorphism approach

Descriptions of the use of mathematics in any empirical science are usually based on the interpretation of mathematics as a collection of abstract languages. Given a real world problem we seek to find an isomorphism between the empirical concepts and propositions and the terms and axioms of a mathematical language. If this can be satisfactorily achieved then the problem can be translated into the abstract language so that a mathematical solution can be deductively arrived at which is then translated back into the real world context. Thus in the field of mechanics the behavior of physical objects is viewed in terms of such concepts as mass, length, time,

force, velocity, density, and a number of other derivative concepts, so that the structure of relations between elements is isomorphic with the structure of operations defined by the operations of algebra, coordinate geometry and calculus. As Coleman states:

"It is this isomorphism which allows the operations of algebra, carried out with real numbers, to substitute for the actual manipulation of physical objects and thus to constitute a useful language for the science of mechanics. The power of algebra and calculus in the theory of mechanics lies in the fact that once the isomorphism is established, then many paper-and-pencil operations with symbols can be carried out which could never be carried out in practice with the objects themselves."<sup>28</sup>

This classical way of using mathematics has been introduced into geographic methodology by Harvey who gives a fuller discussion than is possible here.

The following diagram (Figure 2) sums up this approach to using mathematics.

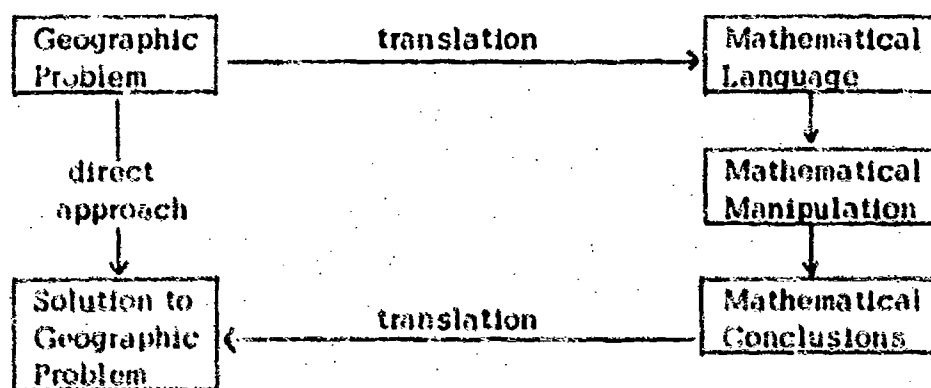


Figure 2. The Use of Mathematics in Problem Solving.  
(adapted from Harvey<sup>29</sup>).

Since the use of mathematics in any given research problem is not usually as explicitly described as in Figure 2, it will help to fix this idea by illustrating the approach with examples from geography. The example we use is Weber's industrial location triangle and Euclidean geometry.<sup>30</sup>



We start with a geographic problem: what is the location that minimizes transport costs given the following information - the locations of a raw material, a fuel deposit and a market and the consumption of raw material and fuel to produce a given quantity of an industrial product. Since the problem is one of location we postulate an isomorphism between "geographical space" and Euclidean geometry and translate the "minimum transport cost" problem into the language accordingly. Thus, the geographical locations of the raw material, fuel, and market become "points" and the distance between them "straight lines". This geometric figure is Weber's familiar "location triangle". Now, if the "relative weights" of the raw material, fuel, and finished product in producing one unit of finished product are also translated into Euclidean space with the "distances" between the three points represented by straight lines proportional to the relative weights we have defined the "weight triangle". This translation now permits the full use of the theorems of Euclidean geometry in a mathematical manipulation that can deductively lead to the solution of the "geographical problem". The theorem that angles at the circumference of a circle from a chord are equal leads to conclusion that the point of interest lies at the intersection of the three "circles of construction" (circles with radii proportional to the respective "relative weights"). This result is then translated back into the real world as the point of minimum transport costs. Thus we have come full cycle in Figure 2. Many other

examples of mapping geographical problems into mathematical languages could be used to illustrate the isomorphism approach. Thus, multivariate problems have been translated into matrix algebra,<sup>31</sup> regionalization problems into a "qualitative" language - set theory,<sup>32</sup> problems of the distribution of urban places into the theory of modified Poisson process,<sup>33</sup> while many problems of statistical inference have used the language of probability theory.<sup>34</sup> The translation from a geographical problem to a mathematical language is not always as smooth as our discussion of the Weber location triangle might suggest. Any translation presupposes that the researcher has assumed an isomorphism between his real world problem and a mathematical language. In the use of inferential statistics this assumption can often be strongly challenged and this has led to what Hogben has termed "the contemporary crisis in statistical theory" or more simply The Significance Test Controversy.<sup>35</sup> The sociologist, Selvin, sums up the criticism of inferential statistics:

"In design and in interpretation, in principle and in practice, tests of significance are inapplicable in non-experimental research".<sup>36</sup>

If these arguments are accepted we are left with the situation that whereas we have identified a research problem - we require to make inferences concerning some empirical evidence - there is no isomorphic mathematical language for use to map into. Selvin recognizes this dilemma and continues:

"Sociologists would do better to re-examine their purposes in using the tests and to try to devise better methods of achieving these purposes than to continue to resort to techniques that are at best misleading for the kinds of empirical research in which they are principally engaged".<sup>37</sup>

In a similar vein but in the specific context of geographical research, Gould has recently argued that the "problem of spatial autocorrelation" -- the lack of independence between locations in space -- may render the use of traditional statistical estimation and inference procedures as invalid in many, if not most, research situations.<sup>38</sup> If the arguments of Servin and Gould are of merit, the researcher would seem to have four alternatives. First he can "muddle through" with the statistical methods already at his disposal either bearing in mind that his inferences must be guarded ones or by developing sampling strategies so as to more closely approximate the assumptions of the method he is employing. Second he can return to non-mathematical approaches on the grounds that there are no mathematics suitable to his problem. This involves employing "the direct approach" (often visual comparison of maps) in Figure 2. A much more sophisticated alternative is to develop a mathematical calculus that does fit the problem at hand. Finally, a more realistic approach, particularly if the researcher possesses a low level of mathematical expertise, is to revert to the non-deductive, "experimental" use of mathematics such as Monte Carlo simulation. These latter two approaches presumably relate to Servin's call "to try to devise better methods" and are the subject matter of the second part of this section.

Languages unavailable: the experimental approach.

If a researcher finds himself in a situation with no available mathematical language, then the most relevant reaction would seem to be to

develop such a language. The classical example of this procedure can be found in seventeenth century research in mechanics where "the continued exploration of changing quantities soon forced scientists to realize that they required a profounder mathematical tool, and they hastened to create it."<sup>37</sup> The situation was such that Newton and others found that they could not translate the concept of "instantaneous speed" into the simple language of functions in coordinate geometry. The result was Newton and Leibniz's creation of differential calculus.

This solution to the unavailability of languages obviously requires a high degree of mathematical expertise - a characteristic previously noted to be rare in social science and geography. However, geographers have looked to this approach to solve some of their problems. Olsson for instance points out that recent studies in spatial perception suggest "that geographers have a need for a geometry which topologists have suggested but not yet invented - a geometry with holes in it".<sup>38</sup> Similarly Bunge in his attempted reformulation of central place theory complains that

"While this theory has great appeal due to its simplicity, it must be admitted that an efficient computation procedure (language) is not available from the mathematicians."<sup>39</sup>

In neither of these two cases has a solution to the dilemma been forthcoming. However, if Olsson's and Bunge's suggested needs are met in the near future it is likely that this will not come about in geography as a new mathematical language but will rather consist of an experimental approach - as has happened in other areas of geographic research where this dilemma has occurred.

Although a dichotomy into theoretical and experimental parts is common among empirical sciences, the application of this division to mathematics seems at first sight surprising. For instance, we have previously, in our discussion of the nature of mathematics, rejected John Stuart Mill's view of mathematics as an empirical science in favor of the more recent interpretation of mathematics as a collection of abstract languages that are independent of real world phenomena. However, despite this interpretation the fact remains that it is possible to carry out "experiments on mathematical objects, such as numbers or equations or polygons."<sup>42</sup> Thus we can define the experimental use of mathematics or experimental mathematics as the method of inferring conclusions from observations on mathematical objects, as opposed to "real world" objects and in contrast to deducing conclusions from postulates within a given mathematical language. Therefore, mathematical experiments can be used when existing languages are not available for solving a research problem. This may be because the research problem is just too complex to be handled by any existing language.<sup>43</sup> However, there are times when an experimental approach may be adopted even though deductive solutions are available. First of all the deductive system may be theoretically practical but not feasible in terms of computer time and costs. Secondly, even if the deductive solution is feasible it may be that a simple experiment might be both quicker and cheaper if only an approximate solution is required. Finally, an experimental approach may be employed because the researcher is unaware of the availability of a suitable language within mathematics. This last reason for adopting an experimental approach is probably the most common in

the social sciences in general and geography in particular. Whatever the reasons the experimental use of mathematics has had an important role in modern quantitative geography and we will briefly review some examples.

The most common experimental use of mathematics in geography has been Monte Carlo simulation, a method originally stemming from work on nuclear energy during the Second World War.<sup>44</sup> It was introduced into geography by Hagerstrand<sup>45</sup> in the early 1950's and has subsequently become very popular, particularly in the work of Morrill.<sup>46</sup> Hagerstrand's work consisted of setting up a framework in which the diffusion of innovations could be studied through time and space. This framework was not translated into any mathematical language for analysis but rather took on the role of a basis for sampling experiments with random numbers which simulated diffusion patterns. Thus we find a different situation to that depicted in Figure 2. Instead of a mathematical language we have an experiment. The new pattern of research is shown in Figure 3.

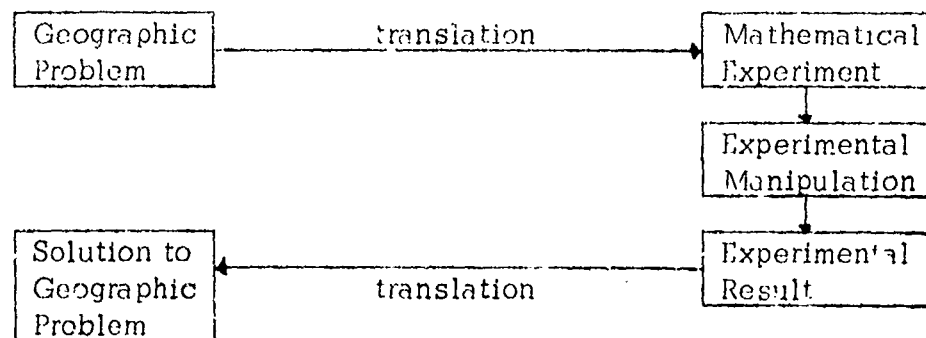


Figure 3. The Experimental Use of Mathematics

Another common method of experimentally using mathematics is iterative procedures. These encompass a wide range of techniques and have become recently represented in geographical research in attempts to solve "the taxonomy problem" in a regionalizing context.<sup>47</sup> In this case the geographic problem is to find the optimum arrangement of base units into regions given some objective function. There is one example of deductively deriving a linkage tree from a similarity matrix but this is for the simplest type of objective function based on the unsatisfactory single linkage criterion.<sup>48</sup> In contrast the most recent approaches involve mapping the problem into an iterative mathematical experiment that tries a large number of possible solutions and chooses that one that best satisfies the objective function. This experimental result is then taken as the solution to the regionalizing problem.<sup>49</sup> Scott's recent work in combinatorial programming demonstrates the necessity of developing principles by which to assess the solutions arrived at through iterative procedures.<sup>50</sup>

There is, however, a very real danger in employing an experimental approach. This is the classical problem plaguing all inductive inference. The researcher can make empirically true statements about the results of his experiments and yet can make statements that are not true if he attempts to generalize to other cases. Two examples from the geographical literature illustrate this nicely - Robinson's attempt to find suitable "weightings" to overcome the areal scale problem in correlation analysis<sup>51</sup> and Porter's approach to finding the point of minimum aggregate travel.<sup>52</sup> As demonstrated by Thomas and Anderson, Robinson based his conclusions concerning a

special case that has no general relevance to the correlation scale problem.<sup>53</sup>

If he had started with a different configuration of areally distributed values his results would have been different. Similarly, while Porter used several point patterns (and hence did not commit the same type of error as did Robinson,) his method of finding the median center was a special case of the particular coordinate axes he used.<sup>54</sup> The utilization of different axes would produce quite different points of minimum aggregate travel. The moral of these two stories is simply that if this approach is to be employed then the researcher should use a well planned experimental design where relevant variables are identified and either held constant or manipulated in the subsequent experiment. An example of such an exercise in the experimental use of mathematics can be found in Blalock's study of the correlation scale problem.<sup>55</sup> This piece of research illustrates well the potentialities of this approach in future research.

In the case of geographic Monte Carlo simulation models similar as well as additional problems arise. First, how many runs of the model must the researcher complete before he attempts to compare his results with the real world. Second, there is no generally satisfactory method of comparing a map of simulated outcomes with a "real world" map. Namely, a mathematical language for doing so has not been invented. Clearly, the experimental use of mathematics solves some of the problems of "no languages available," but a "basic set" of languages must be available or such use of mathematics may create more problems than it solves!



Languages for Geographic Theory?

Although it can claim to have a long pedigree,<sup>56</sup> experimental mathematics have come to the fore, in the physical and social sciences, with the advent of electronic computers. Several advantages can be claimed particularly with respect to the problem of real world complexity discussed earlier.

Bartholomew writes:

"In the past the temptation to trim the model to a form having a tractable mathematical solution has been strong. This situation has been radically altered by the wide availability of high speed computers."<sup>57</sup>

Coleman goes on to claim that "a major stumbling block in the use of mathematics in social science may have been finally overcome."<sup>58</sup> However, Coleman tempers his enthusiasm by pointing out that although the computer may serve as "a substitute for the mathematical deductions",

"the primary disadvantage of this procedure.... is that it never provides a general abstract solution; it provides only particular numerical solutions depending on the prior setting of the model's parameters. Furthermore, when the model is probabilistic, any single run on the computer provides only one case, whereas a great many cases are need to get a picture of the mean value or the overall probability distribution. The computer cannot solve problems in algebra; it can only carry out computations when actual numbers are fed into it."<sup>59</sup>

Coleman's statement, of course, applies with equal force to the simulation models developed to date in geography.

Quite simple the operations and results of mathematical experiments cannot be developed into general models for theory in the way that deductive mathematics can. This basic disadvantage may be far less important than its ability to handle complex situations given the present level of development in

geography. Even the use of deductive mathematics in geography has largely been piecemeal with few attempts to identify an isomorphism between an abstract calculi and a geographic theory. Nonetheless it is instructive to compare how geography has used mathematics in the past with how it might most profitably use it in the future.

Returning briefly to the classical example of the physical sciences, Kline has written:

"...mathematics enables the various sciences to draw the implications of their observational and experimental findings. It organizes broad classes of natural phenomena in coherent deductive patterns. And today mathematics is the heart of our best scientific theories, Newtonian mechanics, the electromagnetic theory of Maxwell, the Einsteinian theory of relativity and the quantum theory of Planck and his successors."<sup>60</sup>

Not surprisingly we have been unable to unearth anything comparable to these examples in geography since both experimental and deductive mathematics have tended to be used to solve "particular" geographic problems with little or no attempts to use them as a "language for theory". Since the evidence from the more developed sciences indicates that this is the role in which mathematics has most to contribute we conclude this chapter by returning to a consideration of two fundamental problems that have limited the usefulness of mathematics as a language for theory in geography.

Perhaps the most acute problem is that in geography a set of concepts has yet to be developed which has some correspondence to a simple yet powerful mathematical language, and at the same time, to observed spatial patterns and behaviors. In general, theory construction, where it has been

attempted, has until recently, kept to the richness and the ambiguity of natural language. This is true, for example, of what some may claim is human geography's only "theory" - Christaller's<sup>61</sup> original formulation of central place theory.

Secondly, geography even lacks a well developed tradition of verbal theorizing. As has been pointed out with considerable skill by Bunge and Harvey, geography has experienced in recent times 19th century methodological controversies - the most rigorous of which centered on Kant's statement about the role of geography in the system of knowledge, exceptionalism - so basic as to question even whether it was logically possible for geography to develop laws and theories. Since these controversies, particularly that over exceptionalism, form topics within subsequent chapters, it is important here to note only that the methodological credo of uniqueness dominated the research of geographers for most of the first half of the twentieth century. It should not be surprising to note, therefore, that human geography contains fewer verbal theories (i.e., those stated in a natural language) than most of the other social sciences. This may not be a distinct handicap, but at least it tends to deny mathematically orientated geographers a ready-made source of verbal constructs from which rigorously formulated theory could, potentially, be developed.

These two closely related problems limit the use of mathematics as an "a priori" model in developing geographic theory to different degrees. In the former case the need is to sharpen existing theoretical concepts and thereby strip them of their ambiguity so that they may be mapped into an

abstract language. Dacey's "Geometry of Central Place Theory"<sup>62</sup> serves as an outstanding example of this type of development. That quantitative geographers tend to confer the status, theory, to central place theory is probably attributable to the fact that researchers subsequent to Christaller (Lösch,<sup>63</sup> and Isard,<sup>64</sup> in particular, as well as Dacey) found at least a partial isomorphism between verbal statements of the "theory" and mathematics. The absence of even verbal theories should not be taken as prima facie evidence that mathematics is not extremely useful in a science. In such a case, however, the use should not be piecemeal and ad hoc but part of an overall strategy for developing theory. We will discuss a hypothetical example that illustrates this point.

Much of the early uses of quantification (e.g., correlation and regression) were the seemingly straightforward application to geographic problems of standard statistical techniques. We say "seemingly" because upon reflection they appear to be neither straightforward nor quantitative in any strict sense of the word. A hypothetical yet characteristic example would take the following form: what are the  $x_1, x_2, x_3, \dots, x_n$  in the relation  $y = f(x_1, x_2, \dots, x_n)$ , where  $y$  is, say, the agricultural productivity of small areas. The problem is one of finding a set of  $x_i$ 's which account for variations in agricultural productivity over a set of areas. Or, stated slightly differently, the problem is one of locating the important "determinants" of the given phenomenon. Although the data themselves are numerical, the types of inferences drawn by the early "quantitative geographer" were generally qualitative, e.g., as  $x$  increases,  $y$  will increase. It is clear

that the researcher could have taken as his task the identification of the parameters  $b_1$  in the equation  $y = b_1 x_1 + b_2 x_2 + \dots$  and thereby establish quantitative empirical generalizations, but in very early stages of investigation, such identification might have led to too early formalization. In short, we are suggesting that some of the early "quantitative" work in geography, given the paucity of general theory to guide in the specification of "determinants" was basically qualitative - the location of "relevant variables" which governed or were at least related to the behavior of some other variable. In no way do we mean to disparage the accomplishments of the early "quantitative" geographers who concerned themselves with discovering "determinants of  $y$ ". However, it is important to stress (a) that the isolation of such variables is, at best, only a beginning along the often circuitous route to the development of fruitful theories and (b) that such an isolation should not be interpreted as a satisfactory end-product in the use of mathematics, but precisely that point in a scientific endeavor when its use becomes necessary - namely, the specification of some measurement theory to establish appropriate metrics for the concept viewed as relevant to a proposed theory, the empirical specification and mathematical statement of the relationships between concepts, and the manipulation of the latter so as to yield logical conclusions.

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## CHAPTER II

### "THE EMERGENCE OF QUANTIFICATION IN GEOGRAPHY"

#### Introduction.

The considerable body of literature generated during, and following what has now come to be known as the "quantitative revolution" in geography provides ample evidence that the introduction of mathematical and statistical techniques to the discipline has not been unheralded. It is by no means our purpose to add to that literature yet another discussion of the applicability or non-applicability of quantitative techniques within geography. Rather, the aim of this chapter is to clarify the process by which the members of the discipline made some early and uncertain moves that led subsequently to a more fruitful use of quantification. However, this does not suggest that geography is as yet making effective use of mathematics (see Chapter I). Neither is this an attempt to write a history of quantification in geography. Several reviews of early quantitative work are available, and, although none is as complete as we might desire, the brief summary by Berry and Marble of the early relations of geography and statistics<sup>1</sup> and the review by Gould of quantitative work in geography since about 1950<sup>2</sup> would seem especially helpful in this regard.

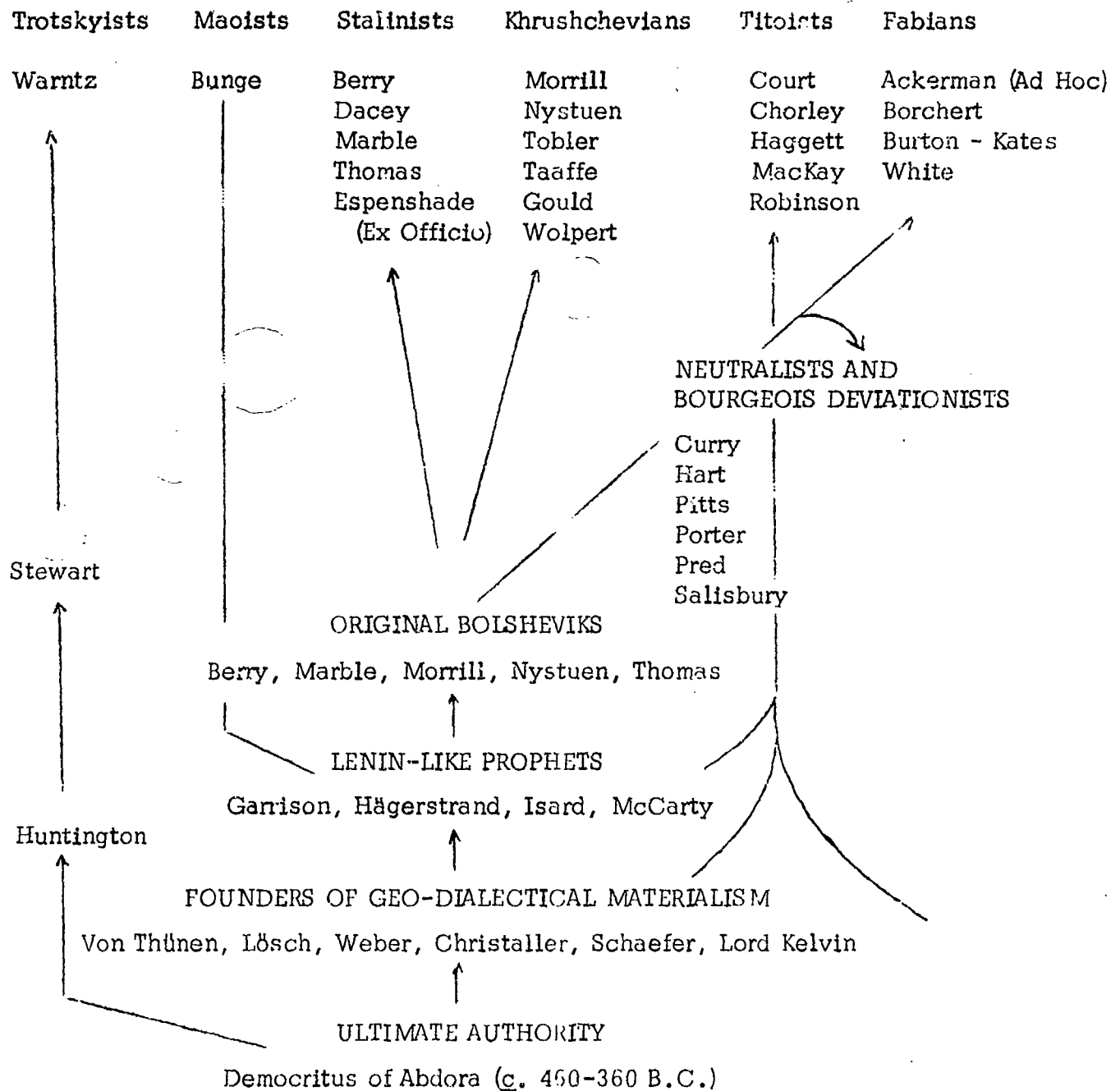
For our part, we will attempt in this chapter to structure the events involved in the rapid intake of quantitative methods into the geographical profession in the late 1950's and early 1960's, and to assess the inter-related efforts at improving research techniques and methodologies.

### The Nature of the "Revolution"

The rapid growth in the use of quantitative methods in geography in the last decade or so certainly has many of the characteristics of a revolution (Fig. 1). It is not clear just who was the first to use the phrase, but after Burton's 1963 article it became common to speak of the "Quantitative Revolution in Geography."<sup>3</sup> Indeed, those of us whose professional lives span even a part of the years of the revolution can attest to the joys and the frustrations of working in "revolutionary times." We can recall the vigorous struggles for power within departments, if not in our own, then through hearsay about the struggles in others. We remember the sharp exchanges and the heated debates at professional meetings, with often-times the sharpest comments being exchanged among persons supposedly on the same side of the imagined quantitative/non-quantitative fence. Some of the more entertaining pieces in our professional literature grew out of the controversies over quantification. The Spate-Berry exchange,<sup>4</sup> the Burghardt-Dacey-Porter series on the spacing of river towns,<sup>5</sup> or Arthur Robinson's editorial classification of geographers into "Perks & Pokes"<sup>6</sup> can be read again for both amusement and insight.

Although some of the events associated with the rise of quantification in geography have been revolutionary in the rapidity with which they followed one another or in the impact they have had on individuals and their work, it is the thesis of this chapter, and indeed of the entire report, that the phrase "quantitative revolution" does not really provide a very adequate characterization of what has been the course of events. The incorporation

# THE STRUCTURE OF THE QUANTITATIVE REVOLUTION



Source: Fugitive Manuscript (circa 1962), authorship, unknown.

Figure 1

of quantitative methods as an integral part of geographic investigations, as indeed in any scientific endeavor, is taken as an established fact. In this sense, the quantitative revolution is over. There is no possibility of a complete reversal in this matter, any more than there was the slightest chance that quantification could be held at some pre-determined level.

However, the process of quantification involves considerably more than the adoption of this or that quantitative technique and once begun it is not really possible to declare that we'll only have it up to a point -- "Quantification is fine, but we ought to draw the line at regression analysis"! The attitude of some early opponents to quantification in geography who felt that "a little bit isn't bad, but we shouldn't get carried away", or that quantification implied the use of a new set of "tools" and who together with Professor Stamp felt that perhaps the profession had already spent enough time and energy perfecting its tools, probably badly misinterpreted what the basic nature of the revolution was all about.

It is only within the past few years (nearly a decade since Burton hailed the end of the revolution) that we are beginning to see writings which attempt to broach the thorny methodological and epistemological issues associated with the "revolution" in geography. Most of these writers recognize that it was the adoption of a new research paradigm, not merely quantification, which has had a most profound influence on the direction of geographic research and

instruction. In a sense, it is this ideological aspect, rather than tactical considerations, of the revolution which is of greater significance. As Burton has noted, the more significant feature of the revolution was not quantification per se, but the adoption of a new style and direction of research, in other words, a new research paradigm.

This is not to deny that quantification itself played a most significant role in the adoption of a new paradigm -- in fact, it is impossible to see how the two can be separated. The growth of an increasingly scientific, increasingly nomothetic approach to geography encouraged, and indeed necessitated the increasing use of quantitative techniques. Conversely, the need to make effective use of the power of statistical analysis and mathematical statements encouraged a rethinking of what constitutes meaningful research in the discipline. In fact, with the advantage of hindsight, it is not altogether certain whether even some of the most ardent early supporters of quantification were fully aware of the consequences of their actions. Just what quantification entailed was only appreciated more fully somewhat later. During the process of incorporating this new paradigm, the profession viewed and used quantitative techniques in a variety of ways and toward a variety of ends. A major aim of this work is to examine some of the various functions of quantification in the development of modern geography.

#### The "Methodological" Revolution

The very idea of a revolution presupposes the existence of an old order which is to be overthrown. Certainly the development of quantitative

methodology in the discipline was not established without a struggle and the critics were both verbal and adamant. However, with a few notable exceptions,<sup>8</sup> the critics argued on the basis of opposition to the techniques, rather than the more fundamental issues of the methodological and philosophical basis of the new paradigm.

Those most concerned over the new developments in geography, often feeling that the significance of their own research was being questioned, found it convenient (and perhaps also easier) to attack the techniques being used -- condemning them for being used as ends in themselves, or for their misuse, etc. -- rather than the central methodological issues arising from the introduction of a new method of research. In attempting to categorize the major opponents to quantification, Burton<sup>9</sup> paralleled the line of reasoning expressed more generally above (Chapter 1) when he identified: (i) those who thought that the whole idea was a bad one and that quantification would mislead geography in a wrong and fruitless direction, (ii) those, like Professor Stamp, who argued that geographers had spent too much time perfecting their tools and should get on with some practical work, (iii) those who believed that statistical techniques were suitable for some kinds of geography but not all geography because there were certain things which could not be measured, or, a variation of the theme, that variables are too numerous and complex for statistical analysis, (iv) those who believed that the techniques were incorrectly applied, that ends were confused with means, that the discoveries were not novel ("quantifying the obvious"), and finally, (v) those who felt the quantification was alright, but that quantifiers were not.

Some of these criticisms concerning the inappropriate or improper use of statistical procedures were undoubtedly and undeniably apt. It is certainly true that some of the earlier attempts to employ quantitative techniques erred greatly either by misapplying the techniques or by too extravagant claims about the power of such techniques. The latter was probably due in part to the exuberance of scholars involved in what they saw as "new" lines of research (a sort of frontiersman syndrome) and the former probably owing to the fact that many were experimenting with quantitative techniques for the first time and lacked experience in their proper application. But these attempts involved something much more fundamental than the use of techniques. As Burton rightly noted, "Dissatisfaction with idiographic geography lies at the root of the quantitative revolution"<sup>10</sup>, although that dissatisfaction was not always well articulated initially.

Whatever the shortcomings of those first attempts, and however falteringly progress was made, it is true that the view of geography as a quantitative, model-building science became widely accepted. Some would argue that, owing to the generally meagre state of geographical theory, this is not what geography is, even though most agree that this is what it should strive to become.

In pursuit of this objective of a scientific geography, students of the discipline have come to use quantitative methods more and more frequently in conducting their research. In the preface of his book, Introduction to Mathematical Sociology, Coloman has stated that "In the development of any science two things are crucial: systematic empirical study and systematic



conceptual elaboration".<sup>11</sup> Geographers, coming from a discipline with a very strong empirical tradition, found ample opportunities for attempting to systematize and structure their observations, and to refine their measurements. However, refinements of these sorts were likely to be fruitless, unless clearly related to systematic conceptual elaboration. The scientific methodology proved a double-edged sword, and it became obvious to those concerned that the increased rigor in empirical study afforded by the adoption of quantitative methods would be undermined unless a more robust conceptual framework were provided which could define which observations and measurements were of significance.

Such methodological transitions in disciplines are unlikely to be smooth. T. S. Kuhn's description of the process of paradigm change argues not for a rational, logical acceptance of new and better ideas by members of a discipline.<sup>12</sup> Rather, he sees the development of a series of crises in present modes of operation and the emergence of a "community of scholars" who have espoused a new way of carrying on science. This new community has rules of behavior different from the previous "community" and a considerable measure of misunderstanding is likely to be present. It is not clear that geography fits the Kuhn model of paradigm change as well as Chorley and Haggett seem to suggest.<sup>13</sup> Nonetheless, we can probably assume with Kuhn that the process will be as much sociological as it is the "winning over" of adherents on the basis of the superiority of particular methodological positions.

### The Process of Paradigm Change

We can assume as a starting point that the structure of events involved in the introduction and acceptance of quantitative methodology in geography was not unlike that experienced in other disciplines. In viewing this process, we will aim to describe the temporal and functional relations of a certain set of events. We have argued that this process has entailed considerably more than the increased use of this or that quantitative technique. What set or sets of events are involved? We might suggest that we are concerned, among other things, with the following:<sup>14</sup>

1. Obviously we are interested in quantitative activities themselves, the kinds of quantitative activities engaged in and how they were used. We might distinguish three types of quantification that ideally could concern us here. The first is simple measurement or enumeration, the presentation of data in tabular or cartographic form for which there is a long history in geography. Second is the analysis of data according to recognized statistical procedures. The rapid rise in this kind of data manipulation is normally what is meant by the "quantitative" revolution. Third, is the use of the formal logic of mathematics to state propositions and develop logical structures. In many ways developments in the mathe-matization of geography are only just beginning. Our concern is initially with the second of these three types, although progress from one to the other is a major objective.
2. The revolution would be expected to be accompanied by significant developments in theory, progress in theory articulation,

new kinds of problems attacked, old problems reassessed and reformulated. We will pay particular attention to how these developments relate to quantification.

3. We would also want to integrate into our structure various kinds of institutional developments, the nature of professional training, the number and outlook of new members of the profession, changes in publication outlets, changes in the professional power structure. As suggested, T. S. Kuhn has argued persuasively that scientific revolutions are strongly sociological and we accept this as a point of departure for examining the profession of geography as a changing social institution.
4. Finally, disciplines do not operate without some contact with happenings in other fields. Developments in scientific methodology generally, giving rise to new ways of looking at old problems, can be traced and linked with developments in geography. Many would accept the general proposition that in this regard geography has been a borrowing and a lagging science.

The rapid rise in the use of more formal types of quantification in geography in the 1950's is therefore seen as part of a more basic change in the whole pattern of geographic inquiry, the full ramifications of which are only now being examined. This process involves changes in several different facets of a discipline's structure and modes of behavior. Moreover, other disciplines have gone through and are going through such processes and we might well obtain a

first approximation to a structure of the quantitative revolution by examining the structure of the revolution as experienced in other fields.

Spengler in reviewing the progress of quantification in Economics has identified three broad stages.<sup>15</sup>

- a. a first stage in which the work of the field is essentially qualitative, thought often sprinkled with statistical information.
- b. this is followed by a second stage in which concepts become well defined and well demarcated and hence translatable into quantitative or quantifiable terms.
- c. then in a third stage efforts in quantification lead to a clearer definition of concepts.

This view covers a much longer time span than is intended in this chapter, but it might be well to speculate briefly on the appropriateness of such a model to geography.

Certainly, the first stage can be recognized as an apt description of most geographical writing from the early geographical compendia of Ritter or Morse to modern regional textbooks and descriptive journal articles. Geography has had close and early ties with statistics, especially as a means to describe and characterize countries and regions. Witness, as one example, the marriage of statistics and geography in the American Geographical and Statistical Society, the name apparently reflecting an accurate description of the function of the American Geographical Society in its early years.<sup>16</sup> Statistical information in the form of data on area, population, temperatures, heights of mountains and even more complex

data incorporating densities or averages has long been standard fare in geographic treatises.

With no intention of slighting the efforts that apparently lay back of the development and acceptance of such descriptive devices, we can pass on to stages two and three as being more critical for purposes of structuring the "quantitative revolution". Spengler regarded this as a crucial part of the development in economics.<sup>17</sup> He traces the slow steady increase in quantification during the 19th century. However, after about 1890 there were several decades of rapid growth and a burgeoning of interest in and use of quantification. Spengler doesn't identify it as such, but this might well be the equivalent of the "quantitative-revolution". He does list a number of features of this period that are highly suggestive for geography. These are 1) methodological differences between the several schools (of economics) became sufficiently composed and concepts sufficiently clarified to make quantification easier; 2) quantitative techniques had improved and were continuing to improve rapidly;<sup>18</sup> 3) the number of persons familiar with such techniques, or capable of adapting them to economics had greatly increased; 4) the number of journals which gave space to "technical economics" and to papers using mathematics greatly increased; 5) finally, several capable and enthusiastic leaders came to the forefront, persons who were enthusiastic about the role that mathematics and advanced statistics could play in the development of economic theory.

Reviews of the development of quantification in other disciplines have

described the progress of quantification in similar terms. A key feature seems to be two parallel and subsequently interacting developments. One is in statistical treatment itself generally moving from simple numerical description to more complex multi-dimensional measurement concepts and also to various types of numerical manipulation leading to statistical inference. The other line of advance is in the development of concepts, theories, and models, generally moving from verbal to graphical to mathematical. Under normal circumstances these two lines of development proceed together and mutually reinforce one another. In the early stages, the crude verbal conceptualizations are little more than highlighted or illustrated by simple descriptive-statistics, whereas in later stages the field operates with complex analytical systems. It is the process of moving from one to the other that is at issue here.

#### Antecedents

Most observers would probably agree that the years from roughly 1954 to 1965 can be identified as the core period of the quantitative revolution. Although we repeat our caution that the full implications of the revolution are yet to run their course, we can agree that the span of a decade or so is a remarkably short time for a discipline to have witnessed a change in the dominant attitudes toward its methods of inquiry. We won't argue strongly for any particular beginning or ending event, but for convenience we might select 1954 as the year that saw William Garrison's first experimental seminar in quantitative methods at Washington<sup>12</sup> and also

several specifically quantitative papers at the AAG meetings in Philadelphia.<sup>20</sup> While Burton's claim in 1963 that the "revolution is over"<sup>21</sup> may have seemed slightly premature, we can accept both Burton's assessment which was based on statements by leaders in the discipline and the findings of La Valle, McConnell and Brown<sup>22</sup> that by 1965 quantitative courses in major departments and quantitative articles in leading journals were of sufficient number to mark the end of an era. Whatever the limits chosen, this was the period of rapid intake of formal statistical procedures into geography and, although, as has been argued, this is only one of the several elements of the changed method of geographic inquiry, it undoubtedly was a key feature.

There, of course, had been several instances of the use of formal statistical procedures in geography prior to the period in question and we might well ask why they were not followed immediately by others. A 1936 article by John K. Rose utilizing linear correlation and regression<sup>23</sup> is often cited as an instance of this earlier work and there were others. For example, two papers also utilizing linear correlations were read at the 1935 (St. Louis) meetings of the AAG.<sup>24</sup> We will examine below the ways in which particular quantitative methodologies were utilized in these early efforts and our conclusion is that the particular use to which the quantification is put does make a difference on the likelihood of a sustained follow-up, but that use can only be judged by reference to the more general philosophical and methodological stance of the field at that time. We might try to contrast the intellectual and institutional circumstances in the 1950's with those of earlier periods to get some insights regarding the conditions attendant upon

the revolution.

A consideration of the literature of the field during the several decades preceding the 1950's suggests several hypotheses. The similarity with Spengler's observations about economics and with Kuhn's model of paradigm change is obvious and, frankly, we have examined the literature of geography with these models in mind.

1. A "quantitative revolution" was in progress in the earlier period because the field had a very unstable methodological position.

In the decades of the 1920's, 30's, and 40's, a relatively small band of professional geographers were thoroughly occupied with attempts to clarify a view of their field. The battle over environmental determinism was still being waged. A variety of modifications, such as "probabilism", "geography as human ecology", "stop and go determinism" had been proposed but these had not resolved the issue and probably had further compounded the methodological confusion. Sauer's "Morphology of Landscape" had raised another alternative but had not succeeded in resolving the uncertainty. The thoroughgoing logic of Hartshorn's presentation of the chorographic view in 1939 began to gain adherents and eventually brought some measure of agreement to the field. Some would say that this represents a pre-paradigm condition in which a science attempts to identify the common methodology and form in its work. The point is that by the early 1950's some of these differences in points of view had been resolved and clarified and methodological debates were less rampant.

A corollary of the methodological uncertainty following on the demise of environmental determinism was the reluctance to engage again in the



sort of broad generalizing that characterized that epoch in American geography. Warntz found the bold generalizing of the environmental determinist heyday to be a period of excitement in the field and a time during which its standing in the academic ranks was high.<sup>25</sup> However, the reaction to this was a tendency to fall back on careful, constrained, descriptive work. This view is expressed, for example, in V. C. Finch's Presidential Address when in a well reasoned essay on the methodology of the chorological or regional school he states:

"the classes of explanatory hypotheses employed in chorological reasoning are not presumed as yet to have wide ..... validity. They are particular for limited sets of conditions. In this respect the regionalist has learned caution from the anthropogeographical school of a generation past."<sup>26</sup>

This constrained, particularistic view is at odds with the sort of generalizing that is involved in numerical data reduction and statistical inference.

2. Revolution was more likely to occur in the 1950's when new ideas and new practitioners were coming into the field in greater numbers.

In the immediate post World War II period there was a significant rise in the number of persons entering the profession. Taking number of new Ph.D.'s in geography as the measure, data collected by the National Academy of Sciences - National Research Council indicates 10 to 15 doctorates a year in the years prior to World War II, dropping during the war, but rising in the early 1950's to 35 to 50 each year.<sup>27</sup> We have not attempted to gather detailed biographic data on new entrants into the profession at various periods. Our presumption is, however, that greater

numbers coupled with the variety of wartime experiences of various members of the profession created conditions wherein the knowledge of and the receptivity to new ideas was more favorable.

We would add to this some changes in the institutional arrangements affecting the Association of American Geographers. In the first several decades of the Association's existence, it was the prevailing practice that younger scholars making presentations to the profession had to be introduced or presented by an existing member. This practice apparently lasted up until World War II. There is no way of knowing whether this inhibited the growth of quantitative work. That it did not do so completely is clear from the fact that the paper by Samuel T. Bratton noted above was given on the occasion of his "introduction" to the profession. It is quite probable, however, that such a practice did limit experimentation. Following 1945, there seemed to be a general "loosening up" and expansion of this, the major professional group. At about this same time, also the younger and quasi-revolutionary Society of Professional Geographers was merged with the older organization.

3. The acceptance of quantitative methods of inquiry became more likely in the 1950's because the discipline was charged with failure to progress.

During the 1950's several kinds of institutional threats were perceived by the profession. One involved the closing out of geography at major universities or at least the threat that geography would no longer be supported. Another kind of attack came from the foundations that in several cases made

decisions not to support geography because it was intellectually sterile and seemed not to be very useful. Interestingly, these challenges to the secure place of the discipline in the academic world came at a time when enrollment in geography was increasing, perhaps even faster than that of other disciplines.<sup>28</sup>

4. The late acceptance by geography of the scientific paradigm that includes quantification relates to its isolation from the major streams of thought in natural and social science.

This point has been discussed by others so we shall only sketch it here for sake of completeness. The major external contacts of geographers in the United States have been with geology or with history. As Harvey points out, these fields maintained the historical mode of inquiry longer than most disciplines.<sup>29</sup> Few geographers were very thoroughly trained in what might be termed normal science as represented by physics or chemistry, or even normal social science as represented by psychology or economics.

Tending to fortify isolation was the "exceptionalist" tradition in geography that was perhaps present in vague form among geographers early in this country, but which received explicit treatment in Hartshorne's 1939 essay on the Nature of Geography. To have such persuasive philosophical arguments presented in support of a position that comes naturally to any group of scholars was apparently the force that kept much of geography isolated for such a long time in this country.

Alan Pred, in reviewing the background of Hägerstrand's pioneering work with diffusion of innovations, makes the point that geography in Sweden had not been isolated from other social sciences, but had close institutional and intellectual ties with anthropology, economics, and other fields.<sup>30</sup> This may account for the fact that Swedish geographers, though comparatively few in number, are prominent among the pioneers of quantitative geography.

5. Interest in quantitative methods did not develop significantly in geography until theoretical ideas or concepts were introduced that called for particular kinds of quantitative acts.

We have indicated above that developments in the use of quantitative methods proceed concurrently with developments in theory and concept formulation, each contributing to the other. We expect this to be true for geography and it is a point that requires elaboration in some detail. In the following pages we will take a few selected cluster areas of closely connected problem-concept-technique and attempt to show some elements of their concurrent advance. The ones we have selected are among those that were prominent in the early years of the "quantitative revolution".

#### Areal Association (Correlation and Regression)

LaValle, McConnell and Brown in their previously mentioned survey of the expansion of quantification in American geography found that ".... simple and multiple regression-correlation analyses comprise the bulk of recent statistical application."<sup>31</sup> In the period 1954-1965, covered by their survey, 70 percent of the uses of statistical procedures in dissertations and 64.5 percent of the use in published articles was regression-correlation analysis. Our purpose here is to take a more detailed look at the circum-

stances attending the early uses in geography of this important group of statistical procedures.

The hypothesis is that the early prominence of correlation-regression techniques in the evolution of quantification represents the convergence in the early 1950's of, 1) a philosophical position that areal co-variation was a central problem in geography, 2) a methodological stance favorable to the search for morphological laws derived from pattern co-variation, and 3) familiarity with the tools and techniques of cartographical and statistical correlation. Conversely, it would follow that the absence of this tripartite convergence is the reason why the few sporadic uses of the technique of correlation-regression in the earlier years had not borne immediate fruit.

As early as 1905, Herbertson claimed for geography "the wider concept as the science of distributions."<sup>32</sup> In a more elaborate and well-known statement, De Geer stated that "geography is the science of the present-day distribution phenomena on the surface of the earth."<sup>32</sup> He goes on to elaborate:

"In order to attain this end, a synthesis of distributions, therefore, geography has to work over certain parts or certain sides of its material, in the first place to investigate the many simple relations of distributions or partial distribution syntheses. The study and establishment of distribution relations in any special case will in its turn have required a comparative investigation of at least two distinct distribution phenomena."

This seems a clear call for the study of the correspondence of two or more areally distributed phenomena. Hartshorne's 1939 study dismissed De Geer's view as representing too narrowly a view of geography as the "Where of things"<sup>34</sup>. That this was not exactly De Geer's meaning seems

clear from his statement that "distribution phenomena must be regarded as abstract qualities -- i.e., general qualities or circumstances in the material objects -- and the object of Geography in its very nature is non-material and abstract".<sup>35</sup> In elaboration, however, Hartshorne's views seem to be closer to De Geer's. Explaining the distribution of a particular phenomenon is not, in Hartshorne's view, the main purpose of geography, nor is explaining the location of a particular feature. Rather, "the facts concerning the areal differences in these (distributed) phenomena must be studied in their areal relations, that is, their significance to the area as determined by their relations to other phenomena of the same place, and by their spatial connections with phenomena in other areas".<sup>36</sup> Twenty years later in further clarification, and perhaps some modification of these views, Hartshorne talked in terms of "simple integrations of phenomena over area" and espoused the usefulness for generic studies of examining statistical co-variation over area".<sup>37</sup>

Without attempting a more complete reconciliation of the views of De Geer and Hartshorne, it is safe to conclude that by the early 1950's one of the main objectives of geographic study had come to be the explanation of distributions through the discovery of accordant relations between areally distributed phenomena.<sup>38</sup>

Although there is undoubtedly a considerable degree of overlap in the writings between the philosophy of a discipline and its methodology, as Harvey makes this distinction, the above is directed primarily at how

geographers saw the objectives and purposes of their field. Equally important in the development of the correlation-regression cluster were some methodological developments that provided a clearer, if not also a fresh view of how the field might operate. Indeed, means and ends can both function as the engine for change. Probably, many geographers of the late 1940's and early 1950's were in basic agreement with the objectives of geography as stated by Hartshorne, but most failed to find guidelines for the conduct of their own work. Simpler, more straight forward statements of purpose and method may have had a greater impact on the way geographers went about doing their work than did the more oft-quoted methodological treatises.<sup>39</sup> In following up on these simpler statements geographers found success and satisfaction and this led to a subtle alteration of objectives.

It is often claimed that the twin underpinnings of the areal association correlation-regression work are De Geer's philosophical statement above and Schaefer's methodological statement "Exceptionalism in Geography".<sup>40</sup> Schaefer argued against a unique methodological position for geography. Rather, he claimed that the procedures of systematic geography were in principle no different from that of any other social or natural science. "Science is not so much interested in individual facts as in the patterns they exhibit. In geography the major pattern producing variables are, of course, spatial."<sup>41</sup> Schaefer continues, "assume for instance that two phenomena are found to occur frequently at the same place.

A hypothesis may then be formed to the effect that whenever members of one class are found in a place, members of the other class will be found there also."<sup>42</sup> When sufficient tests of such hypothesis are provided, they become laws utilized to explain situations not yet considered.

Schaefer's methodology had an impact on two groups of geographers. One was at the University of Washington where William Garrison and a group of younger geographers were attracted not necessarily by the areal association techniques, but by the more general proposition that geography's modes of operation were not unlike those of other social and natural sciences. This led them to be among the pioneers in several types of quantitative work in geography (see below). The other was at the University of Iowa where Schaefer was a faculty member for a period of years in the late 1940's and early 1950's. Schaefer's methodology found explicit expression in the work of Harold McCarty and his associates who made use of correlation-regression to test hypotheses of the "where x-there y" form.<sup>43</sup>

Once the philosophical position and methodological view favorable to areal association had been widely accepted it was quite to be expected that one or more individuals familiar with the technique would see that the statistical procedures of correlation-regression were useful tools for this purpose. Correlation had become widely used in a variety of disciplines by the 1930's and 1940's. In agricultural economics, in marketing, in educational psychology and measurement, (to mention just a few of the fields with which early quantitative geographers had contact) correlation-regression



had become a well established analytical procedure and was standard fare in statistics courses. Association with colleagues in other fields (for example, McCarty's work with Davies, a statistician) or students coming into geography with statistical training in other areas seem to be among the ways in which explicit knowledge of these tools entered geography. This same familiarity with statistical procedures, of course, had happened earlier (e.g., J. K. Rose's paper cited above)<sup>44</sup> but had failed to take hold without the concurrent acceptance of a favorable methodological and philosophical stance.

#### Uses of Correlation-Regression

The uses to which correlation-regression was put in these early attempts seem to fall into three categories: 1) testing of simple descriptive hypotheses, 2) ranking variables in a multi-variant situation, 3) finding new variables through the mapping of residuals. Although later workers have been critical of using the technique for these purposes, they seemed consistent with the needs of the field at that time.

Geography by virtue of its early ties with exploration and with natural sciences has had a strong inductive bias to its work. Moreover, the field had only meager development of theory, so that explanations for any pattern of distribution tended to rely either on simple intuitive logic arising from observation or on generalizations about the linkages between phenomena derived from the topical field concerned with that class of phenomena. Neither of these was very satisfactory, the first subject

to the accusation of not being scientific, the second to the charge of superficial borrowing. Statistical comparison of the mapped pattern of a variable of interest with that of another variable chosen logically, or intuitively or from the literature and then judging this comparison in terms of significance tests, while not answering both these criticisms, did lend an aura of scientific, objectivity to geographic work.

The use of this procedure can be seen in several of the early quantitative papers. In McCarty's words the methodology was as follows:

"select one factor at a time and measure the extent of its association with the phenomena of the problem. These individual elements might be derived from any source whatsoever, from economics, political science, psychology, or indeed the simple observation that some map he had seen reminded the investigator of his problem map."<sup>45</sup>

In his work, McCarty tended to have a closer link with theory (see below). but others followed this eclectic procedure rather closely. For example, the paper by Robinson, Lindberg and Brinkman has been criticized by Burton on just this very point.<sup>46</sup>

Most distributions were also seen to be the result of multiple "determinants". There was the need to establish which are most significant and how are they related to one another. Almost simultaneously with the use of simple correlation for pattern comparison, multiple and partial correlation was employed to determine the importance of different variables. The major findings of the papers were often summarized in a table listing the correlation co-efficients obtained for the tested variables with an indication of the level of significance for each. Interestingly, little attention was given in all of this to the functional form of the relationships.

The regression co-efficients themselves were all but ignored. Early papers assumed linearity, although later on with the growth of computer technology, logarithmic and other transformations were used. An exception to this lack of attention to the functional form of the relationship is provided by an early Berry and Garrison paper.<sup>47</sup>

The technique of mapping residuals was commonly combined with the attainment of correlation co-efficients. One purpose of this rested on the belief that the pattern of residuals from calculated regressions would, when mapped, suggest to the perceptive geographer additional variables which would improve the explanation. This was a simple extension of the use of correlation-regression for the general purpose of finding significant variables in a problem. Geographers held the belief that they were not so interested in general relationships, but must show how these relationships relate to the "real world". Therefore, residual mapping was also used to show where a particular relationship held and where it did not.<sup>48</sup> As we see it now, this is not often successfully accomplished by conventional residual mapping. However, others have had the same need and have used correlation techniques in other ways to achieve this same purpose. J. K. Rose, for example, had this as a main goal and mapped a series of correlation co-efficients rather than residuals from a single regression in an attempt to show the areas in which there is a close relationship between corn yield and rainfall.<sup>49</sup> Robinson has done the same thing in relating population density to rainfall in the Great Plains.<sup>50</sup>

Users of correlation-regression techniques in the 1950's were not unmindful of the many technical problems involved in using these tools for spatial analysis. In rereading some of these early works, technical discussion seems to vastly outweigh substantive findings. The problems of modifiable data units, selection of statistical control points, the form of the residual calculation, and others received much attention in these early papers and certainly many of the critical issues were not resolved for many years.

By the early 1960's, the cluster of work based on correlation-regression had evolved so that two divergent thrusts from this common source can be traced in subsequent work. One continued the search for "significant" variables, the unraveling of interrelated factors, and the testing of hypotheses. This need relies largely on summary correlation co-efficients and as these reached the form of large matrices of co-efficients various factor analytic methods began to be used. Variable pre-selection on the basis of intuitive logic was replaced by inductive and analytic methods. This was facilitated, of course, by the concurrent development of high speed calculating machines.

The other stream has looked at the structure of statistical surfaces using least squares regression to calculate trend surfaces or to estimate distance decay functions. Greater use is made of the form of the regression co-efficient. Residual calculations began to take the form of fitting higher order equations or working with statistical filtering procedures. These developments have received much impetus from similar work in the geological sciences, although they were heralded in some of the very earliest papers

in geography, for example, several of those by Arthur Robinson.<sup>51</sup>

Economic Models: Theory and Quantification?

Many people have tended to associate the quantitative revolution with urban and economic geography. LaValle, McConnell and Brown report "that most analyses employing statistical procedures at either the dissertation or article level (during the period 1954-1965) are in the areas of economic and urban-economic geography. Their data suggest that between one-half and two-thirds of the uses of statistical procedures were in these fields. Considering the popularity of these areas of geographic inquiry during the 1950's and early 1960's this may not be so strikingly a disproportionate emphasis. We can accept the fact that interest in economic location theory and quantification came along at about the same time but it is not clear, just what the connection is. Does the subject matter of economic geography and the availability of statistical data in great profusion lead to greater strides in quantitative work? How did theory and method interact in this particular case?

The use of generally applicable laws of spatial relations in economic and urban geography has a history that pre-dates the quantitative revolution. There seemed to be a period in the 1920's in American geography of active and lively interest in the application of economic principles to some of the problems of geography. Articles containing references to and discussions of von Thünen's land use principles, of Weberian manufacturing location theory or of the principles of geographic specialization of labor appeared in the major journals with some frequency during this period.

Events involving the newly founded journal Economic Geography may be taken as examples of the influences at work in this period. The founding of the journal itself is indicative of an increasing interest among geographers in economic and resource topics. Moreover, the journal was designed to appeal to a wide audience and early issues included articles by authors not classed as professional geographers. An article by O. E. Baker in the very first issue listed six economic principles that could be used, along with the more familiar climate, soils, etc., to explain the world wide distribution of wheat growing.<sup>52</sup> These principles, which included the law of comparative advantage, the effect of distance on production, principles of labor specialization, were stated in a concise verbal form not unlike the common practice in much economic writing of the times. In this particular article there was little explicit connection between any of the several deductions that could be made from these principles and the wealth of empirical information presented in the article in tabular and cartographic form. However, a later article in the same journal by Olaf Jonasson, a Swedish geographer taking his Ph.D. at Clark University, does make an explicit connection between Thünen theory and empirical data on agricultural land rents.<sup>53</sup> Jonasson acknowledges the influence of Baker on his own work and also shows considerable familiarity with German and other continental literature on location theory.

Richard Hartshorne in some of his earliest presentations to the profession uses, in discussions of the location of the iron and steel industry, the method of reasoning from general location principles to the facts of

particular cases.<sup>54</sup> His papers stand in marked contrast to the more common historical narrative approach to the same phenomena in articles both preceding and following Hartshorne's work. Again in Hartshorne the connection between theoretical proposition and empirical test is a weak one utilizing narrative form for the presentation of theory and descriptive examples to demonstrate the validity and general workings of hypotheses.

However, in respect to both of these cases these beginnings of the process of forming links between quantitative investigation and theory formulation were not followed up. Hartshorne himself could claim some years later that these papers brought forth little interest on the part of his geography colleagues, but were mainly of use to persons in other fields. He concluded that this was because the "standort" problem, (the location of an individual factory), was outside the main chorographic tradition of geography.<sup>55</sup>

These early attempts to infuse laws, principles and general statements into economic geography in the 1920's were mainly the efforts of younger geographers, but were not without support from more senior persons. Ray H. Whitbeck, not normally associated with this sort of effort, proposed, for example, the development of a sub-field with close links to descriptive economic geography to be called "Geonomics".<sup>56</sup> "Geonomic material is organized primarily as a body of principles or laws operating under the influence of geographic conditions." This call for a body of laws went unheeded, although similar calls were made by others in the ensuing couple of decades. In part this occurred because such proposals were almost totally without

guidelines for empirical work, but also because they ran counter to the tenor of the times in geography, a tendency which Sauer has labelled "the Great Retreat".<sup>57</sup> Sauer had in mind the retreat from contact with other fields and a hesitancy to engage in studies of genesis and the origin of phenomena which was antithetical to historical study, but some of these tendencies also retarded theory building and generalizations.

These early descriptions of theory pretty largely represented the state of the arts in the mainstream of geography until well into the 1950's. The occasional writer would include a short mention or description of an economic-location principle in an otherwise descriptive treatment. Usually this was little more than "window dressing", but whatever the motivation the mutually reinforcing links between observation and theory were simply not present.<sup>58</sup>

The studies in the 1950's that initiated a change in this condition have been gathered into a single volume and we need not detail them here.<sup>59</sup> Most of these, such as the now classic papers by Kennelly and by Lindberg, constitute specific tests of formal economic-location theory employing largely cartographical and simple tabular tests rather than more formal statistical procedures. This is largely because the theories themselves were stated in the form of graphical space-models, but the point is that developments leading toward the mathematical-empirical language of discourse described in chapter 1 were initiated.



### Subsequent Evolution

By the early 1960's the uses of economic-location theory had become sufficiently widespread so that we can identify several classes of usage. The most widespread was for pedagogic purposes. This involved the description of central place theory, manufacturing location theory, economic base theory, land use competition theory and others in the classroom and eventually in textbooks. John Alexander's textbook Economic Geography published in 1963 contains brief descriptions of these theories as they were viewed at that time.<sup>60</sup> The fact that these descriptions are set off by themselves in one of the last chapters is indicative that they are not well integrated with the main empirical treatment of the text and is also indicative of an uncertainty about their acceptance as proper material for undergraduate instruction. Similarly, the second edition of Thoman's text The Geography of Economic Activity contains formal presentations of these theories whereas the first did not.<sup>61</sup>

The inclusion of these theories in texts and in the classroom, although presented largely in graphical form, served in several ways to further the growth of quantitative work in geography. It exposed a generation of students to a fresh view of geographic thinking, it broke the hold that historical forms of explanation had on the field and substituted functional modes, and it served as one of the sources of greater contact between geography and other social sciences. This does not mean to imply a total reversal of the traditional view that theories are tested in the laboratory before appearing

in the classroom. It does suggest that instructors sensing a need to provide a touch of generality and a structure to their classroom presentations seized on these graphical models that had been lying largely unused on the fringes of geographical thinking for several decades and employed them in the classroom almost contemporaneously with their use in research. Because of this the connection between theory and empirical observation was initially weak and only slowly have these links been welded more firmly.

A second use of economic-location theory was as a source of hypotheses for statistical testing in the manner described in the preceding section with the tests and the theoretical deductions corresponding in only a crude manner. This is best seen in the early monograph by McCarty and his associates.<sup>62</sup> McCarty, an economist by training, was well grounded in the classics of location theory. His approach was to use this theory to develop specific hypotheses, e.g., "in order to have a steel mill an area must have a coal mine", which are then tested by statistical methods of correlation. Further progress is obtained by adding in "additional variables that are presumed to be found wherever the original problem variable exists". McCarty summarized his methodology,

"problems were recognized as appearing in the uneven areal distribution of various types of manufacturing. Hypotheses purporting to offer explanations for the unevenness of these distributions were drawn from a variety of sources, mainly economic theory and a few studies of resources and technology. In all cases, however, it was necessary to measure not only the volume of various kinds of manufacturing in various areas, but also the characteristics of the phenomena with which those quantities of manufacturing are presumed to be associated. In this case, as in others, the selection of hypotheses was materially affected by the characteristics of the data..." 63

This later point is critical to the use of economic theory in this type of study. Data availability and the need to order data into forms that meet the requirements of a particular kind of statistical test have meant that the convergence between specific theoretical deductions and empirical verification has not progressed very far. The range of statistical procedures with which geographers of this time had had some experience was very limited, so that if formal statistical procedures were to be employed, and this was viewed as an important goal, then the form of theoretical statements were altered considerably in any empirical test.

A third type of connection between economic theory and quantitative methods was the explicit testing of theory by numerical-quantitative means.<sup>64</sup> The papers by O. Lindberg and R. Kennelly are specific tests of Weberian manufacturing location theory.<sup>65</sup> The empirical tests are in tabular or graphical form and even though Kennelly includes a mathematical statement of the MTP (minimum transport point), he did not attempt a mathematical determination of such a location in the case of his problem, the Mexican steel industry. Rather, he employed the Launhardt graphical method. Computational algorithms for this problem were not developed until several years later.<sup>66</sup>

Other examples of the manner in which early verification of theory ultimately achieves a greater convergence with formal statistical testing procedures are provided by central place theory, (reviewed in chapter 9) and by land use theory. As suggested above, von Thünen models of

agricultural land use were familiar to geographers well before the onset of the quantitative revolution and tabular verification of some theoretical deductions had been attempted. Reformulations of classic land use theory in mathematical terms became available to American geographers through the works of E. Dunn and A. Lösch in the 1950's.<sup>67</sup> Economists such as Henderson and Fox developed large linear programming models of agricultural land use patterns incorporating von Thünen concepts and the results of these models began to be cited in geographical journals and discussed in seminars and courses. Garrison reviewed these and other studies in a series of articles and he and Morrill attempted a linear programming approach to the study of wheat and flour trade.<sup>69</sup> Along similar lines, Stevens pointed out that the "dual" of traditional linear programming formulations of land use can be interpreted as the "location rents" of von Thünen.<sup>69</sup> This influenced geographers to attempt quantitative tests of this proposition, as for example, in the paper by Maxfield.<sup>70</sup> Several of the more important early quantitative papers by geographers were directed more at providing empirical evidence for significant additions and modifications to classical theory, rather than simple tests of theory as formulated.<sup>71</sup>

#### Spatial Interaction

By early 1960, the twin concepts of areal association and spatial interaction had been accepted by many as representing the essential points of view of geography. We don't wish to elaborate this point, nor to trace the origin of these ideas, nor to pursue their obvious similarity to the older concepts of site and situation. Yet the fact that studies of spatial interaction in a great variety of contexts were increasing in geography during

the period under review has important ramifications for the growth of quantitative work. As with the areal association-correlation cluster, these developments represent a variety of influences.

One of the more important of these influences was the coming into prominence of the nodal or functional region idea. G. W. S. Robinson briefly traces the growth of this concept.<sup>72</sup> Robert Dickinson's book, City, Region, and Regionalism which appeared first in 1947, can be used to illustrate how this concept contributed to the growth of quantification in geography.<sup>73</sup> Dickinson was interested in presenting material on the city as a focus of human activity and which views the city-region as a fundamental characteristic of society. He acknowledged some of the material as being marginal to geography as it was practiced at that time, but nonetheless Dickinson's book was read and used by geographers. The impact of the book was to place studies of cities and their hinterlands, town-country relations, trade area delimitation, etc. within the main stream of geographical inquiry during the succeeding decades. Dickinson included extensive treatment of the work of sociologists such as Kolb, Galpin, Park and Burgess, reviewed the scattered works by geographers on trade areas and because of his familiarity with the German literature provided one of the earliest statements in English of Christaller's Central Place Theory. In addition to Dickinson's book, we could mention the concept of "areal functional organization" developed by Robert Platt and some of

his students, and although the association with concepts in outside fields is not as great, the impact on geography was similar.<sup>74</sup>

The point is that efforts such as those by Dickinson, Platt and others brought geographers into closer familiarity with the work of sociology, economics, marketing, and other fields that had developed various kinds of quantitative treatment of these topics. One kind of quantitative activity was the use of gravity models, either as summary data reduction devices or as initial hypotheses in studying town trade areas. By the mid-1940's, through the work of writers such as Reilly, Stewart, Zipf and others, gravity models had become well established in these fields. Gravity model applications in geography were therefore quick to come. Howard Green, for example, reported to the 1952 AAG meetings in Washington on the use of mathematical rules in hinterland determination<sup>75</sup> and his study of the New York - Boston hinterland was an often used example of regional delimitation in the 1950's. By the later part of the 1950's, geographers responding to the aforementioned desire for generality in classroom presentation, were including various forms of the gravity model as hypotheses as well as general descriptive statements of findings. Research reports and discussions of these models in the geographical literature proceeded with equal vigor, although most were in journals peripheral to geographers rather than in the *Annals*, *Economic Geography* and other mainstream quarterlies.<sup>76</sup>

In contrast to the other two clusters reviewed above, the connection between model and quantification in the case of the gravity model has been

very close. Goodness-of-fit tests in the form of scatter diagrams or occasional least squares regression were attempted with the earliest gravity model work. Almost invariably, remarkably good results were obtained and this no doubt served as an encouragement to further efforts of a similar nature. Good results were to be expected given the fact that gravity formulations are, as someone has said, merely empirical generalizations with little basis in theory. The terms of the model are capable of wide interpretation, allow for empirical estimation of critical constants, and are meant to be valid for aggregate populations -- all characteristics that made them very popular. The models provided geographers with experience in model calibration and numerical generalization.

One or two geographers expressed considerable concern over the use of gravity models.<sup>77</sup> They were concerned with the implications of borrowing models from the physical sciences and using them so unthinkingly for modelling human events. This very proper concern directed at gravity models tended, however, to be directed at all quantitative work. The fear that reducing things to numbers would destroy the richness of the human experience in geographic investigation was widespread among early opponents of quantification.

Indeed, subsequent developments have tended to discredit gravity models on the very grounds that they had little or no basis in human behavior. As human geography has moved to more explicit modelling of decision making and behavioral processes, and has desired less aggregated results,

the crude analogies to simple physical models has proved unsatisfactory. They were replaced initially by probabilistic forms of the same general classes of models and now by more complex decision models (see chapter 6). It seems clear, however, that gravity models provided an important impetus to quantitative work. In the 1950's and early 1960's when human geography was predominantly concerned with spatial outcomes and structural properties, these models enabled those geographers anxious to employ quantitative methods with some easily understood activities and ones that also provided immensely satisfactory results.

Many of these same general comments apply as well to a derivative type of quantitative work - the calculation of potential surfaces - but this work has a somewhat separate origin and at least in part has led in a different direction subsequently. Potentials of population are calculated by summing individual gravity model values for a series of points and creating a continuous surface. Such surfaces were introduced into the geographic literature by the Social Physicist, John Q. Stewart, and were developed in the next two decades almost exclusively by Stewart and by William Warntz who worked closely with him.<sup>78</sup>

Potential surfaces have been linked with spatial statistics and centrography through the work of Warntz<sup>79</sup> and David Neft.<sup>80</sup> Neft's monograph provides a review of these developments and we need not repeat them here. Early proponents attached theoretical implications to the potential idea but it was not clear just how this all worked and the models



have remained peripheral to the main advance of theory formulation and empirical test. However, as quantitative descriptive devices, as for example in the early use by Clarence Harris,<sup>81</sup> potential models played an important early role, especially in providing seemingly satisfactory results in applying quantitative methods to popular geographic problems.

### Conclusion

By the mid-1960s, these several trends of development were sufficiently strong so that "quantitative geography" was becoming the "normal" way to do geography. This does not mean that the use of formal statistical methods characterized all, or even a very large share of geographic scholarship. As we have emphasized, this is not the sole characteristic of what we have continued to call for sake of brevity, if not clarity, the "quantitative revolution". We have emphasized the mutually reinforcing changes in the (1) aims of geographic scholarship, (2) in the concepts and tools by which this is carried on, and (3) in the norms of teaching and research carried on by members of the discipline. We can conclude this brief history of the "revolution" by noting some of the social and institutional consequences of these changes that became evident during the 1960s.

The early proponents of quantification in geography were a small band of pioneers held together by a common interest in applying quantitative methods to geography. At national meetings, papers employing quantitative methods regardless of substance were frequently placed together in a small session. Quantitative geographers formed a minuscule but distinctive sub-group within the profession with all the features of

internal competition and external solidarity characteristic of such groups. As the minority became the majority, in intent if not in accomplishment, the common bond weakened and quantitative geography began to go off in a number of different ways. As the number of different statistical-mathematical procedures has proliferated, as the lines between substantive theory and quantitative method have been welded more firmly, as an interest in spatial form has given way to a greater interest in process, as the successful use of statistical-mathematical techniques have required greater technical sophistication, it became increasingly meaningless to speak of quantitative geography or of a quantitative geographer.

Yet in one sense this is not quite correct. We began to see within the discipline in the 1960s the beginnings of a structure to carry on geographic work under the new paradigm. This emerging structure might be viewed as an hierarchical arrangement with three levels. At the first level are the great bulk of professional geographers who possess a working knowledge of statistical methods and a familiarity with the mathematical models in their field of specialization. LaValle, McConnell and Brown have shown the growth in Ph.D. granting departments offering courses in quantitative methods. Mathematical modelling and statistical testing became standard fare in graduate training and with the growth in faculty positions during the 1960s, this first level of quantitative competence became widespread. As the overall sophistication of the field grows, one can expect a concomitant increase in the range and technical ability manifested in this first level of competency.

At the second level are the quantitative specialists in individual departments. They have more than the normal level of training or familiarity with statistics and mathematics; they probably teach a quantitative methods course; and their papers are oftentimes cited as exemplary applications of quantitative methods to geography. Most of the geographers identified early as quantitative geographers operated at this level. Some of these same individuals continue to do so, while others have shifted their interests and operate quantitatively at the first level. A few have crystallized their interests and operate at the third level.

At this third level of quantitative competence are the creators of quantitative methods; those that work on the development of new languages of quantitative discourse (see Chapter 1 for examples), those that devote full time to technical problems of statistics or mathematics, that possess a high level of ability and may even hold appointments in mathematics, or statistics, or operations research departments. The number of persons at this third level will always be small, but their appearance is testimony not only to the maturing of the discipline, but to the fact that the "quantitative revolution" is not a one-time event, but is itself an impetus for change and restructuring of the discipline.

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83. With sufficient data it would be instructive to study the formal and informal communication links in the profession in the late 1950s and early 1960s. Some may be able to recall the beer hall seating arrangements at professional meetings. More formal communication links might be described by the circulation lists of the Discussion Paper series that began about this time (the University of Washington series began in 1958, for example), or by the citation structure described in William Bunge, "The Structure of Contemporary American Geographic Research", Professional Geographer, May 1961, pp. 190-23.
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## CHAPTER II

### SOME EPISTEMOLOGICAL PROBLEMS IN GEOGRAPHY

#### Epistemology

Epistemology is that branch of philosophy which investigates the origin, nature, methods of acquiring and limits of human knowledge.

The aim of methodology..is to describe and analyze these methods, throwing light on their limitations and resources, clarifying their presuppositions and consequences, relating their potentialities to the twilight zone at the frontiers of knowledge. It is to venture generalizations from the success of particular techniques, suggesting new applications, and to unfold the specific bearings of logical and metaphysical principles on concrete problems, suggesting new formulations. It is to invite speculation from science and practicality from philosophy. In sum, the aim of methodology is to help us to understand, in the broadest possible terms, not the products of scientific inquiry but the process itself.<sup>1</sup>

For our purposes it is that body of rules or principles and methods by which one acquires scientific knowledge. Thus we view an epistemological problem in geography as one associated with the ways in which geographers go about (or might potentially go about) developing and testing geographic theory. When there is agreement upon the procedures followed, both within a discipline and the larger scientific community, there are no epistemological problems - or at least they are unperceived.

#### Theory:

In referring to theory, rather than proceeding by formal definition it may be more helpful to give a standard sketch of a theory at this point. This sketch would run something like the following (after Suppes,<sup>2</sup>): A theory in

science consists of two parts. The first part is an abstract logical calculus or syntax. In addition to the vocabulary of logic, this calculus includes the primitives (or undefined terms) of the theory. The logical structure of the theory is fixed by stating a set of axioms or postulates in terms of its primitives. In many theories primitives can be thought of as theoretical concepts like "atom" or "electron" which can not be related in any simple way to observables. In other theories primitives may initially be thought to be undefinable and yet turn out to be derivable in terms of the primitives of some other theory. Such primitives may be thought of as "relative primitives".<sup>3</sup>

The second part of the theory is a set of "correspondence rules" or a semantical system that assign(s) empirical content to the logical calculus by providing what are often called "empirical interpretations" or "coordinating definitions" for at least some of the primitives and defined symbols of the calculus. "It is always emphasized that the first part alone is not sufficient to define a scientific theory, for without a systematic specification of the intended empirical interpretation of the theory, it is not possible in any sense to evaluate the theory as part of science, although it can be studied simply as a piece of pure mathematics" (Suppes,<sup>4</sup>).

The above "standard sketch" of a theory seems simple enough. However it is deceptively simple, primarily because there is no simple procedure for giving coordinating definitions for the terms in a theory.

For example, in specifying these definitions in actual practice the scientist almost invariably finds himself referring to some other theory, if only one of measurement.<sup>5</sup> Furthermore, the necessity of submitting the theory to empirical test might also "bias" the selection of "correspondence rules" if some operational definitions are either more familiar or easier to work with. In this regard Kaplan<sup>6</sup> presents a humorous yet illuminating example:

There is a story of a drunkard searching under a street lamp for his house key, which he had dropped some distance away. Asked why he didn't look where he had dropped it, he replied, "It's lighter here!"

Considerations such as these lead Suppes<sup>7</sup> to conclude that no simple response can be given to the question, "What is a scientific theory?"

Are we to include as part of the theory the well-worked-out statistical methodology for testing the theory? If we are to take seriously the standard claims that the coordinating definitions are part of the theory, then it would seem inevitable that we must also include in a more detailed description of theories a methodology for designing experiments, estimating parameters and testing goodness-of-fit of the models of the theory. It does not seem to me important to give precise definitions of the form: X is a scientific theory if, and only if, so-and-so. What is important is to recognize that the existence of a hierarchy of theories arising from the methodology of experimentation for testing the fundamental theory is an essential ingredient of any sophisticated scientific discipline.

Most philosophers of science probably would not disagree with Suppes' conclusion, but would simply stress that the sketch provided above should not be construed as a description of the actual process by which theories are, or even should be, formulated. Instead it is

an account of the logical or structural characteristics of theories. In a discipline, such as geography, which is still in the early stages of theory development, such an ideal statement of the characteristics of a theory may not correspond closely with the state of theory in the discipline.

#### Reconstructed Logic and Logic-in-Use

Kaplan<sup>8</sup> in his philosophical analysis of the methodology of behavioral science draws a distinction between what he calls "logic-in-use" and "reconstructed logic". The former is descriptive of the "cognitive style" in the research of scientists which is more or less logical and the latter refers to the philosopher's of science explicit reconstruction of a logic-in-use. Reconstructed logic is applied to the scientific product of an inquiry while logic-in-use is that which guides the actual process of inquiry. To be sure, reconstructed logics can become or at least influence the logic-in-use. The logics-in-use in the sciences are affected by the state of knowledge of the discipline, by the stage in a particular inquiry, and by the special conditions associated with a particular problem. Not only are there many logics-in-use but there can, likewise, be many reconstructed logics. However, Kaplan focuses his attention upon the most widely accepted reconstruction of science, the "hypothetico-deductive method", which he argues is the application of contemporary reconstructed logic (that presented as the "standard sketch" of a theory) to the products of science which may be deserving of the status "theory". According

to the hypothetico-deductive reconstruction, the scientist arrives at a consistent, non-redundant set of postulates which describe the interrelations between phenomena of interest. These postulates are arrived at either by inductive logic, shrewd guesses, intuition or some combination of same. From this postulate set the scientist deduces consequences (at least some of which will be observable); he then tests the observable consequences by experiment, and so confirms the postulates. If the deduced consequences are rejected empirically, he modifies or replaces the postulates suspected to be faulty, and so continues.

Such a reconstructed logic is not meant to be a description of what is actually being done by scientists; instead it is an idealization of the logic of science. Kaplan,<sup>9</sup> for example, is critical of this reconstruction because it treats hypothesis (postulate) formation and modification as extra-logical. In this reconstruction, he argues:

the most important incidents in the drama of science are enacted somewhere behind the scenes. The growth of knowledge is surely basic to the scientific enterprise, even from a logical point of view. The conventional reconstruction presents the denouement, but we remain ignorant of the plot.

(Kaplan<sup>10</sup>)

No doubt one of the reasons for the failure to elaborate on hypothesis formation in the hypothetico-deductive method is that no set of distinctive rules of inductive inference has been developed which does not lead to logical inconsistencies.<sup>11</sup>

In geography, as well as in social science in general, there is thus considerable danger in construing a particular reconstructed logic as a logic-in-use appropriate to a discipline in its earliest stages of sophistication both substantively and mathematically,<sup>12</sup> particularly if it provides "rules" which apply primarily to the statement of the results of scientific inquiry rather than to the process of inquiry itself. In this regard, most philosophers of science would agree with Rudner<sup>13</sup> that their domain is primarily limited to providing and scrutinizing the logic germane to the "context of justification or validation" - the rationale on which science bases its acceptance or rejection of its hypotheses or theories - rather than that pertaining to the "context of discovery" - the process by which a scientist comes to discover or entertain a hypothesis or theory. Although no one has yet demonstrated that there is or could be such a thing as a logic of discovery, a few philosophers, notably Hanson<sup>14</sup> and also Kaplan<sup>15</sup>, maintain that such a logic is possible. Hanson's view<sup>16</sup> does not claim that "facts" can be logically arranged so as to lead to new hypotheses in a naive Aristotelian sense nor does he propose a set of epistemological rules in a Baconian or any other fashion.<sup>17</sup> Instead he goes beyond Carnap's belief that inductive logic can be a basis for scientific methodology and pragmatics (Carnap,<sup>18</sup>) and suggests that a logical structure exists in addition to deduction and induction which will enable the scientist to make a rational choice



(although only a plausible one) of the type of hypothesis that is worth entertaining and empirically testing. Hanson in a manner similar to Kaplan suggests that this is the dominant logic-in-use.<sup>19</sup> Since none of the objections against a logic of discovery are decisive,<sup>20</sup> if we accept that geography is no, in the terminology of Kuhn,<sup>21</sup> in a period of "normal science but in the "pre-paradigm" stage of development; then it would seem wise for geographers to keep abreast of the continuing debate in philosophy of science circles over the "logic of discovery"." In fact, Polya's fascinating How to Solve It<sup>22</sup> and recent work in the computer simulation of thought processes, such as Feigenbaum and Feldman<sup>23</sup> suggest that at least heuristic rules of discovery are indeed possible.

Concepts:

Since a theory is only as good as the laws upon which it rests and the laws express connections between concepts, it follows that poorly defined concepts not only bring into question the meaning of law statements but also the meaning of whatever deductions may be forthcoming from them. This is readily accepted. However, it is not as readily appreciated that the converse is equally important -- that a "good theory is needed to arrive at the proper concepts". This dilemma is what Kaplan<sup>24</sup> calls the "paradox of conceptualization" and, since geography has developed only few generally useful and easily measurable concepts, the nature of paradox deserves our attention. The paradox is well illustrated by Kuhn when he discusses

how theories (and the concepts of which they are comprised) guide one's selection of experiential data. In illustrating differences between Aristotelian and Galilean perspectives on the pendulum, Kuhn<sup>25</sup> writes:

....When Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second a pendulum.... Seeing constrained fall, the Aristotelian would measure (or at least discuss - the Aristotelian seldom measured) the weight of the stone, the vertical height to which it had been raised, and the time required for it to achieve rest. Together with the resistance of the medium, these were the conceptual categories deployed by Aristotelian science when dealing with a falling body. Normal research guided by them could not have produced the laws that Galileo discovered.

A further discussion of this paradox in the context of classification is presented in a later chapter. However, the point we wish to emphasize at this juncture is that some semblance of a rudimentary theoretical structure, no matter how crude, must exist in a scientific discipline in order to guide in the selection of the observational phenomena with which the practitioners of that discipline will concern themselves. A further refinement of the discipline's concepts should then lead to the further articulation of theory, and so on. Thus as Harvey<sup>26</sup> states (in part by way of quoting Kaplan,<sup>27</sup>):

It is, fortunately, one of the characteristics of the history of science, that as knowledge of our subject-matter increases and as our theories become more sophisticated and more reliable, so appropriate definition systems become easier to construct. Thus 'the closure that strict definition consists in is not a precondition of scientific enquiry but its culmination'.

In other chapters of this monograph, we have occasion to discuss specific concepts and terms<sup>28</sup> that have been, and are being, employed

in attempts at law and theory formation in quantitative geography.<sup>29</sup>

However, it is appropriate to indicate briefly, by way of introduction to these later chapters, which of the types of terms distinguished by philosophers of science have found widest application in quantitative geography.

Although there are numerous classifications of the terms used in science in the literature of the philosophy of science, the one we utilize distinguishes between three types: observable, dispositional and theoretical.<sup>30</sup> An observable term poses no difficulty. It is simply an entity or property (of an entity) which is ascertainable by direct observation. A dispositional term also is reasonably straight-forwardly described as some entity which has a potentiality for manifesting some property. For example, to describe an entity, say sugar, as soluble is to assert that sugar has the potential to dissolve, not that it necessarily will dissolve under any given condition. Following Rudner,<sup>31</sup> theoretical terms can be roughly distinguished from dispositional terms insofar as the former refer to "nonobservable or nonmanifest characteristics of non-observable entities" while the latter refer "in standard cases to nonobservable or nonmanifest characteristics of observable entities." Theoretical terms include such terms as "electrons", "ego", "marginal utility" and "mass". Strictly speaking, for a term to be called a theoretical it must appear in a theory. Therefore, a term like "mermaidic complex" although perhaps implicitly

definable as a set of nonobservable properties (e.g., the desire to swim in the nude, to sit on slippery rocks in large rivers, etc.) of a nonobservable entity (the mind), is not a theoretical because it does not appear in any theory.

Both dispositional and theoretical terms have received considerable attention from philosophers of science. It is now generally agreed that, even though a full definition of these types of terms is even in principle impossible, they can be given a "partial interpretation" in terms of observables by what Carnap<sup>32</sup> has called "correspondence rules" by way of "reduction sentences".<sup>33</sup> As Kaplan<sup>34</sup> indicates, these procedures do not really provide explicit definitions for theoreticals in terms of observables, but rather mark the conditions for their application. Braithwaite<sup>35</sup> develops two simple theories each containing theoretical terms, in which he demonstrates that it is possible to provide explicit definitions of the theoretical terms by considering them, as Russell suggests, to be "logical constructions"<sup>36</sup> out of observable entities. However, in doing so he is also able to demonstrate that his theories are rendered as nothing more than alternative ways of describing the empirical generalizations they purport to explain. Braithwaite concludes by stating that a "theory which it is hoped may be expanded in the future to explain more generalizations than it was originally designed to explain must allow more freedom to its theoretical terms than would be given them were they to be logical constructions out of observable entities",<sup>37</sup>

and "theoretical terms can only be defined by means of observable properties on the condition that the theory cannot be adapted properly to apply to new situations."<sup>38</sup> A concrete example of Braithwaite's contention is provided by Hempel.<sup>39</sup>

Suppose that the term 'temperature' is interpreted, at a certain stage of scientific research, only by reference to the readings of a mercury thermometer. If this observational criterion is taken as just a partial interpretation (namely as a sufficient but not necessary condition), then the possibility is left open of adding further partial interpretations, by reference to other thermometrical substances which are usable above the boiling point or below the freezing point of mercury; this permits a vast increase in the range of application of such laws as those connecting the temperature of a metal rod with its length or with its electric resistance, or the temperature of a gas with its pressure or its volume. If, however, the original criterion is given the status of a complete definiens, then the theory is not capable of such expansion; rather the original definition has to be abandoned in favor of another one, which is incompatible with the first.

The import of the above should neither be taken as a license for the scientist to fail to define his terms nor as bestowing scientific legitimacy upon quests for the "proper", "correct", or "true" definitions of terms. As Bergman<sup>40</sup> points out:

A concept is neither true nor false, only propositions are. A concept is neither valid nor invalid, only arguments are. Yet there is a distinction of "good" and "bad" among defined descriptive concepts. To have a name for it, I shall say a concept either is or is not significant. A concept is significant if and only if it occurs, together with others, in statements of lawfulness which we have reason to believe to be true. It follows that some concepts are, in an inherently vague sense that cannot and need not be made precise, "mere

significant than others. For instance, a concept that occurs only in one or two tentative and isolated laws is "less" significant than one that occurs in a well-established theory of considerable scope.

Instead, the importance of theoretical terms in a science depends upon the stage of development of that science. In a field which is still primarily concerned with establishing empirical generalizations, the failure to precisely define terms by way of observables would be disastrous; it would render research findings incomparable and hence lead either to breakdowns in communication between researchers or to false inferences and, most important it would result in a wasting of those empirical generalizations that were discovered.<sup>41</sup> However, in advanced stages of science the introduction of theoretical terms in postulates from which a number of empirical generalizations can be deduced is almost inevitable. It is at this stage of development -- namely, once a corpus of theory has been developed -- that Braithwaite's view of lending only temporary observational interpretations or as he calls them, "implicit definitions," to theoretical terms appears to have considerable heuristic value. "It stimulates the invention and use of powerful explanatory concepts for which only some links with experience can be indicated at the time, but which are fruitful in suggesting further lines of research that may lead to additional connections with the data of direct observation."<sup>42</sup> Unfortunately, as we indicate below, geographers seemingly have the tendency of viewing many of their supposedly fundamental concepts as theoretical terms even in the absence of theory and hence have been prone to only "partially interpret" them rather than defining them and then "getting on with it." This

is particularly true of the term distance.<sup>43</sup> Instead of being satisfied with defining it in terms of some standard metric and then searching for empirical generalizations, quantitative geographers have been increasingly prone to claim that statements containing the term "distance" are important in explaining human and physical phenomenon. This may be true, but when distance is defined differently in such statements, it is not true that "distance" is necessarily a theoretical term utilized by geographers in their explanations. Quite clearly it is not -- it is a rather long list of different observational and dispositional terms.

One further aspect of a theoretical term is that it may have no meaning when it is divorced from the theory of which it is a part. Stated differently, whatever meaning a theoretical term has may derive solely from the role or function it performs in a theoretical structure. In the geographic literature, Harvey<sup>44</sup> provides an example of this "instrumental" role or, if you will, the contextual meaning of a theoretical term in his contrasts between the terms "economic rationality," "satisficing behavior," and "perception." Economic rationality is well defined only in the sense that it is an abbreviation utilized to denote the behavioral assumptions upon which economic theory is based. Satisficing behavior and perception have no such contextual meaning.

#### Concepts in Geography

Although we endorse the spirit of the final sentence of Harvey's excellent Explanation in Geography -- "By our theories you shall know us," we feel compelled to add "and by our concepts we (geographers) shall know our theories." One failure of quantitative geography already alluded to in our

brief example concerning distance is that many of its practitioners seem to have failed to recognize the epistemological distinctions between the concepts extant in their field. No doubt when and if someone writes the history of the quantitative geography of the 1960's, he will conclude that a preoccupation of the decade was the quantitative description of so-called spatial structure (although it also went under the guise of spatial form or pattern). Furthermore, he will probably note that geographers became quite sophisticated in this task.<sup>45</sup> To be sure, description -- the work of collecting and classifying facts -- is extremely important in any science and as Caws<sup>46</sup> states:

in some sciences it is almost the only objective at the moment, not because the workers in those fields would not like to do other things but because they are still laying an observational foundation. Parts of biology and archaeology are cases in point.

Nevertheless, one might ask to what extent and in what ways has the quantitative "analysis" of spatial structure laid the "observational foundation" of quantitative geography.

There can be little doubt that the research strategy of the early wave of quantitative geographers characterized by ad hoc hypothesis formation and subsequent empirical testing via the classical procedures or statistical inference is gradually giving way to a strategy in which the elaboration and testing of operational hypotheses is deferred until adequate theory is at least partially formulated -- namely, until that time when the theorems in a theory have been deduced or induced and given an interpretation in observational terms. The earlier strategy in hypothesis construction represented a fiddling around that is still characteristic of much social research. For example,



many geographers of the fifties and early sixties searched for and often found some measuring instrument (with which to give an operational definition to a term), proceeded to use it, discovered some relationship between the values measured on several variables, and then usually asked the question, "Into what kind of theoretical structure can I refer this observed relationship?" Fiddling around of this sort may turn out to be of scientific value (1) if the researcher is willing to build back to some theory from which the empirical generalization he accidentally uncovered could be deduced or (2) if he was lucky or perceptive enough to combine his empirical generalization with other generalizations which were logically consistent and could yield, as yet, unobserved deductions.<sup>47</sup> For the most part, however, geographers were quite unwilling (or at least did not venture) to attempt one of these approaches to theory construction. Perhaps the initial reason for this was a lack of any consensus as to what it was geography was to theorize about. As suggested above, it became apparent in the 1960's that quantitative geography set as its explanandum spatial structure. However, in doing so much of the discipline's effort in concept formation was channeled into efforts not to explain empirical generalizations of spatial structure,<sup>48</sup> but to specify the definition of spatial structure itself. In the terminology of this chapter, spatial structure was viewed as a theoretical concept the explicit definition of which in terms of observables must be established before it can be useful as an explanandum. In principle, nothing is epistemologically misguided about this; but there certainly is if one deludes himself into thinking that there is one and only one correct meaning of the term. King,<sup>49</sup> drawing on the work of Dacey, has

suggested that there are at least three viewpoints on spatial structure or form manifest in the contemporary research of geography: those couched in substantive language, coordinate (or space-time) language, and in the perceptual modification of substantive language.

In a substantive language, the individuation of an object is regarded as resulting from the properties which it manifests. In such a language therefore, 'spatial form' would refer to a set of properties ( $p_1, p_2, \dots, p_n$ ) necessary and sufficient to characterize the objects involved.<sup>50</sup>

In the concept of spatial form in a space-time coordinate language, "the individuation of an object results from the position which it occupies and spatial form would refer to this position both in time and space."<sup>51</sup> The perceptual viewpoint of spatial form is the same as that expressed in substantive language except that directly observable properties are replaced with the properties as apperceived. However, from the tone of King's paper, it is clear that he is disturbed that there is no single meaning implied in the quantitative geographer's use of the concept spatial structure. In one sense, his apprehension is justified because, if geographers were to define spatial structure differently, then the problems mentioned earlier are encountered -- difficulties in communication, the increased risk of making false inferences, and the wasting of well verified empirical generalizations. However, provided the three views on spatial structure are recognized as different descriptive aspects or properties of the same "thing" or set of "things," rather than as competing definitions of the same property, and are labeled as such (i.e., each is abbreviated by a different term), then these views are healthy signs. For to insist on only one property or property set as that of interest to the field greatly restricts the

scope of the laws it can discover. Most importantly, King's discussion serves to highlight the general confusion that surrounds the term "spatial structure" -- a confusion stemming largely from the premature conferment of the status "theoretical" to it. In short, a large segment of quantitative geography has placed the proverbial "cart before the horse." Continued effort devoted to the invention of the true definition of spatial structure is not only unsound from a logical standpoint (recall Bergman's distinction between "truth" and "significance") but precludes the laying of the observational basis of the field and hence denies the possibility of having empirical phenomena (or rather empirical generalizations or laws) to theorize about. This derives from the simple fact that empirically testable theory containing only theoretical terms in its deductions or theorems is, by definition, impossible.<sup>52</sup>

Quantitative geographers have not, of course, been solely concerned with providing descriptions of the real world through the various empirical interpretations of spatial form. In a few cases, empirical regularities (some pertaining to spatial structure) have prompted attempts at "theory formation" in the sense that Hempel uses the phrase. As will be indicated in later chapters, the quantitative geographer in attempting to account for these regularities has made good use of theoretical concepts borrowed from the other sciences but, with the exception of the "indigenous" term distance, few theoretical concepts have been invented by geographers which have been useful in explaining phenomena. In part, this is attributable to the still widely accepted dogma that geography is distinguished from the other sciences not by the phenomena its practitioners study but by the "spatial perspective" employed in the study of an phenomenon.

However, it is also the result of failing to recognize the extremely important role played by theoretical concepts in science in general and of confusing empirical and theoretical concepts in research.

"Realistic" Postulates in the Construction of Geographic Theory

We wish to conclude this introductory discussion of epistemological problems in quantitative geography with a brief examination of one aspect of a "cognitive style" in theory and model-building which has been advocated or implied with increasing frequency in methodological works in geography.<sup>53</sup> The "cognitive style" to which we refer is what Kaplan<sup>54</sup> calls the postulational style. Of the cognitive styles discussed by Kaplan, this is the closest to what we referred to in an earlier section as the dominant, contemporary reconstructive logic.<sup>55</sup> Kaplan states:

Emphasis is on the system as a whole, bound together by the chains of logical derivation. Rules for such derivations are explicitly formulated and applied. The foundation on which the whole system is erected is a set of propositions laid down to serve in just this way. These are the postulates; often they are also called "axioms," though in more strict usages this term is reserved for postulates whose truth can be established without appealing to anything beyond pure logic and mathematics. In general, postulates have an empirical content, and their truth is dependent on matters of fact. From the postulates theorems are derived, whose verification indirectly validates the postulates by which they are proved. Interest centers on the independence of the postulates from one another (none of them is a theorem of the system constituted by the rest), and on their mutual consistency (a proposition and its negation cannot both be derived from the set). What is wanted is the simplest set which will suffice for the derivation of the theorems in which we are interested, one which will allow for elegant proofs of the important propositions about the subject matter.

As can be seen, the "system" to which Kaplan refers constitutes a theory, provided at least some of the theorems are testable empirically and none of the postulates (some of which may be tested empirically) are patently refutable.<sup>56</sup>

For want of a more concise term and for purpose of illustration, we will refer to theory which is concerned with the explanation of some aspects of spatial structure (however interpreted) as location theory. Using Harvey<sup>57</sup> as a precedent, attempts at the development of location theory in geography can, in general, be characterized by the types and sources of the postulates utilized. There are two broad types of potential postulates in location theory: spatial postulates specifying the properties of the area or space over which behavior or action occurs and behavioral postulates which are law-like statements describing the behavior of the actors with which the theory is concerned.<sup>58</sup> Likewise, in each theory or potential theory there is at least one conjugate postulate linking behavior with space: termed spatial behavior postulates. For example, in traditional central place theory the spatial postulates are those of the "theory of the firm," and the spatial behavior postulate that of distance minimization. These three types of postulates appear to be present in one form or another in all attempts at deriving location theories, even those couched in the language of probability theory. It is important to note that we are giving a catholic interpretation to behavior so as to achieve generality. Behavior can refer to decision-makers who can be identified empirically or to more abstract behavior such as that of points on a lattice.

Recent probes in the development of location theory have taken two quite different forms. On the one hand, there is the attempt at rendering traditional location theory more explanatory by the incorporation of spatial and spatial behavior postulates which are more "realistic" than those of the "theory of the firm" and distance minimization. On the other hand, there are

the attempts to develop stochastic location theory in which the "behavioral" postulates and resulting processes are embedded in stochastic models of point generating processes and hence not behavioral in a social or psychological sense.<sup>59</sup> Of these two types of probes the one of interest here is the former and the aspect of this postulational cognitive style which we wish to examine from an epistemological standpoint is the notion of "realistic" postulates. It is around this notion that many of the main arguments for more "behaviorally relevant locations theories" revolve.<sup>60</sup>

First, it should be reiterated that all postulational system must contain lawlike propositions which interrelate the concepts of interest two or more at a time and that the propositions must themselves be interrelated (in the sense that they can be logically connected). Ideally, one adopting a postulational style can achieve a completely closed deductive system in which there are a minimal set of propositions taken as postulates, from which all other propositions can be deduced as theorems by purely logical reasoning. In practice, however, if the explanation of propositions with at least partial interpretation in observational terms is the goal, then one must take a completely closed postulational system as the ideal which can only be approximated. Hence, one basic epistemological problem faced by the researcher adopting this cognitive style is one of dealing with less than completely deductive systems. As Blalock<sup>61</sup> states (if somewhat dogmatically):

The need for deductive theories, when combined with the need for testable theories that are sufficiently complex to give really new sights, poses a major dilemma for the theory builder. In order to develop deductive theories, one must ordinarily begin with very simple models that are totally inadequate to mirror the

real world. By adding new variables and hence axioms a few at a time, one can construct more realistic theories by what amounts to an inductive process.

A demand for "realistic" postulates in a location theory can be interpreted, in one sense, as a demand that all postulates in a system be stated in observational terms. This interpretation at least in the extreme form just stated must be rejected since it is a throw-back to very early logical positivism and is tantamount to denying the relevance of theoretical terms (concepts) to progress in science. Even if one were to accept this extreme empirical interpretation of "realism," a classical problem of induction would still "raise its ugly head" -- namely, although "in principle" testable, postulates would, strictly speaking, be untestable because of the impossibility in social science and non-experimental physical science of "controlling for all relevant variables." It is possible to "get around" this latter problem as Blalock<sup>62</sup> suggests by construing postulates as "causal assertions" and by making certain additional assumptions concerning the operation of factors not included in the theory. Given this, it would then be possible to derive from the postulates testable theorems. But,

if the theorems prove false the theory must be modified or the axioms of the theory even abandoned. But if they are true, one cannot claim that the theory has been 'verified' unless all possible competing alternatives can be rejected.<sup>63</sup>

It appears obvious, then, that construing a realistic postulate as which is empirically true poses considerable difficulty in an empirical sense as well as being overly restrictive from a theory-building standpoint. There is, however, another interpretation of a realistic postulate which is less restrictive epistemologically and not left to the subjective realm of intuition. Why not

convey the status of "realistic" to any postulate or potential postulate in a theory unless the practitioners in that area of inquiry can point to either an accepted theory containing a theorem which contradicts it, or to observational evidence that contradicts it? This is clearly a less restrictive definition of "realistic" and one which is in accordance with Kuhn's<sup>64</sup> view of "normal science" in that it encourages the cumulative development of theory -- a theorem of one theory becomes the postulate of another, and so on.

With regard to recent attempts to develop a location theory in a postulational style which is more explanatory than the traditional, we conclude that (1) realistic postulates in the sense of empirical validity are not only unnecessary but are unlikely to lead to very powerful theories (i.e., theories derived from such postulates will generally be of narrow scope,<sup>65</sup> and (2) predilection over empirical truths of postulates can only impede the development of location theory by encouraging the premature closure of our definitional system through extreme operationalism.<sup>66</sup>



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3. Rudner, R.S., Philosophy of Social Science (Englewood Cliffs, N.J.: Prentice-Hall), 1956, pp. 52-53.
4. Suppes, op. cit. 2, p. 56.
5. In the case of measurement Caws, P., "Definition and Measurement in Physics." in C.W. Churchman and P. Ratoosh (eds.), Measurement: Definitions and Theories (New York: John Wiley), 1959, Chapter 1, has stated "it has not always been clear whether the term means an operation involving an observer and a more or less complex apparatus, or whether it means the number that emerges as a result of such an operation-- whether, in other words, a measurement produces a result or an operation produces a measurement."
6. Kaplan, op. cit. 1, p. 11.
7. Suppes, op. cit. 2, pp. 63-64.
8. Kaplan, op. cit. 1.
9. Kaplan, op. cit. 1, p. 10.
10. Ibid.
11. See Hempel, C.G., Aspects of Scientific Explanation (New York: Free Press), 1965, pp. 3-79.
12. Geography has sometimes been characterized as "long on facts but short on theory." We do not doubt this characterization, but hasten to point out that many of the "facts" may be useless since they were collected with schema in mind other than the elaboration of theory.
13. Rudner, op. cit. 3, pp. 5-6.
14. Hanson, N.R., Patterns of Discovery (Cambridge, England: Cambridge University Press), 1958.

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16. Hanson, op. cit. 14, 1961.
17. The Baconian procedures are well reviewed in Harvey, D., Explanation in Geography (New York: St. Martins), 1969, pp. 32-35; and will not be presented here.
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22. Polya, G., How to Solve It (Garden City, N.Y.: Doubleday Archer paperback), 1957.
23. Feigenbaum, E.A., and J. Feldman (eds.), Computers and Thought (New York: McGraw-Hill), 1963.
24. Kaplan, op. cit. 1, p. 53.
25. Kuhn, op. cit. 21, pp. 120 and 122.
26. Harvey, 1969, op. cit. 17, pp. 305-6.
27. Kaplan, op. cit. 1, p. 77.
28. Although the words, concept and term, strictly speaking refer to different domains--thought and language, respectively--we refer only to "term" in the following even when we might preferably use the word "concept." We think this will cause no confusion.

29. Even though an important area of debate, space prohibits a discussion of the perceptual basis of concept formation and the various distinctions and relationships between precepts, concepts, and terms. For such a discussion see for example Caws, P., The Philosophy of Science (Princeton, N.J.: D. Van Nostrand), 1965.
30. This trichotomy is an approximate distillation of the views of Hempel, C.G., "Fundamentals of Concept Formation in Empirical Science," International Encyclopedia of Unified Science, Vol. II, (Chicago: University of Chicago Press), 1952; Hempel (1965, op. cit. 11); Caws (op. cit. 29); and Rudner (op. cit. 3).
31. Rudner, op. cit. 3, p. 23.
32. Carnap, R., "Testability and Meaning," Philosophy of Science, Vol. 3, pp. 420-468 and Vol. 4, pp. 1-40, 1936-37.  
  
Carnap, R., "The Methodological Character of Theoretical Terms," in H. Feigl and M. Scriven (eds.), Minnesota Studies in the Philosophy of Science, Vol. I, (Minneapolis: University of Minnesota Press), 1956.
33. A detailed summary of such procedures is also presented in Hempel (1965, op. cit. 11, pp. 187-210).
34. Kaplan, op. cit. 1, pp. 57-76.
35. Braithwaite, R.B., Scientific Explanation (Cambridge, England: Cambridge University Press), 1953, Chapter 3.
36. Russell, B., Mysticism and Logic and Other Essays (London, 1918), p. 155.
37. Braithwaite, R.B., op. cit. 35, p. 76.
38. Ibid., p. 76.
39. Hempel, 1965, op. cit., pp. 204-5.
40. Bergman, G., Philosophy of Science (Madison, Milwaukee, and London: University of Wisconsin Press, 1966), p. 50.
41. This latter point is made succinctly and forcefully by Bergman, op. cit. 17, p. 63.
42. Hempel, 1965, op. cit. 11, p. 206.

43. Distance has been given a host of "partial interpretations," such as "time," "cost," "friction," etc. See Olsson, G., Distance and Human Interaction: A Review and Bibliography (Philadelphia: Regional Science Research Institute), 1965.
44. Harvey, D., "Conceptual and Measurement Problems in the Cognitive-Behavioral Approach to Location Theory," in K.R. Cox and R.G. Colledge (eds.), Behavioral Problems in Geography: A Symposium (Evanston: Northwestern University Press), 1969, pp. 35-67.
45. For a review of this literature, see King, L. J., "The Analysis of Spatial Form and Its Relation to Geographic Theory," Annals, Association of American Geographers, Vol. 59 (September), 1969, pp. 573-595.
46. Caws, P., op. cit. 29, p. 91.
47. Dubin, R., Theory Building (New York: Free Press), 1969; Coleman, op. cit.
48. There were, of course, some important exceptions; notably those efforts to "explain" the so-called negative exponential law of population density out from central cities.
49. King, L. J., "The Analysis of Spatial Form: Some Theoretical and Applied Shortcomings," mimeo, 1970.
50. Ibid., p. 2.
51. Ibid., p. 3.
52. It should be apparent that we are assuming for the moment that geographers wish to develop theories containing statement of spatial structure which are the deductive consequences of a set of postulates (law statements).
53. Although there are important epistemological distinctions between theories and models and indeed the debate over the functions of models in science is an important epistemological problem in itself, we feel that models have been sufficiently well discussed in the geographic literature so as not to warrant our opening of, yet, another Pandora's box. See for example, Harvey (1969, op. cit. 17), Olsson, G., "Inference Problems in Locational Analysis," in K.R. Cox and R.G. Colledge (eds.), Behavioral Problems in Geography: A Symposium (Evanston: Northwestern University Press), 1969, pp. 14-34, and Haggett, P. and Chorley, R.J., (eds.), Models in Geography (London: Methuen), 1967, p. 26.

54. Kaplan, op. cit. 1, p. 261.
55. Kaplan also discusses five other "cognitive styles" in behavioral science: the "literary," "academic," "eristic," "symbolic," and "formal." It is beyond the scope of this monograph to discuss these; but, we think the geographer would find Kaplan's discussion of these rewarding.
56. By "patently refutable" we mean that a postulate cannot be shown to be false with a high probability. This, of course, can only be established inductively.
57. Harvey, 1969, op. cit. 44, pp. 116-122.
58. Olsson, G., op. cit. 53, p. 17.
59. This is not to say that a stochastic location theory which is behavioral in this sense cannot be written.
60. See for example Cox, K.R. and R.G. Golledge (eds.), Behavioral Problems in Geography: A Symposium (Evanston: Northwestern University Press), 1969, pp. 1-13.
61. Blalock, H., Theory Construction (Englewood Cliffs, N.J.: Prentice-Hall), 1969, pp. 3-4.
62. Ibid., p. 11.
63. Ibid.
64. Kuhn, op. cit. 21.
65. For a discussion of the scope of a theory, see Bergman, op. cit. 40, pp. 33-34.
66. For a now classical argument concerning the serious logical difficulties of operationalism, see Carnap (1936-37), op. cit., Section 4).

## CHAPTER IV

### QUANTITATIVE DESCRIPTION IN GEOGRAPHY

#### Geography as Description

Only a year before Burton declared the quantitative revolution over in geography, Professor Darby, in an article entitled "The Problem of Geographic Description," wrote that despite the varieties of opinion expressed concerning the nature of geography, few would deny the statement that "geography is concerned with the description of the earth".<sup>1</sup> As Professor Darby himself points out later in the article, there are a variety of modes of description which may be applied. Anyone who has pursued the geographic literature cannot help but be impressed with the various modes of description which are encountered. There is, for example, the term "mere" description usually accompanied with an implied snarl, which is apparently used to refer to works of description for description's sake. Perhaps hoping to avoid this onerous label, others have resorted to "explanatory" description, "interpretive" description, or even "enumerative" description, all of which presumably are superior to mere description. The essential difference in these terms would appear to be the function of description. We might infer from the literature that perhaps there are two basic forms of description: impressionistic and objective. The distinction should be clear, at least to those who maintain either that geography is an art or that geography is a science. It would seem that geographers, particularly of the latter-day variety, are none too schooled in the use of the former. While

it is clear that description may be impressionistic and Sauer, among others, argued for its more widespread use; it seems that "there are... relatively few attempts to convey an impression of what... countrysides look like".<sup>2</sup> While this state of affairs may indeed be lamentable, others argue that it is precisely the need to get away from such "subjective" descriptions that has brought about the plea for more "objective" studies. It is acknowledged that such an adoption of the more objective form of description cannot get around the problem of selectivity. On this point Harvey writes:<sup>3</sup>

Geographic description, it is clear, cannot eschew selectivity or value judgments. But their incorporation does not give the geographer license to do just as he pleases... To confront a priori image with intuitive impressions smacks uncomfortably of tautology. Herein lies the greatest danger in admitting intuitive impression as a legitimate part of geographic description. Even the best geographer may have secret predilections for seeing what he wants to see--a comforting deja vu that short-circuits understanding and appreciation.

In searching out adequate descriptions, therefore, the geographer requires control--control over the collection and selection of information, control over its manipulation.

But, one might well ask, control for what purpose? So long as the function of geographic description was to "convey an impression" of some unique portion of the earth's surface, a call for control was clearly both unrealistic and uncalled for. It would be tantamount to ordering the artist to paint pictures using only a prescribed range of colors--it destroys the essence of the exercise. Certainly the works of Vidal de la Blache and others in the French tradition of geographic research illustrate the need for incorporating a wide range of descriptive devices. Vidal recognized the need for:

a concept which would encompass spatial and social identity, a label which would designate those groupings whose economic, social, spiritual, and psychological identity had imprinted itself on the landscape. *Genre de Vie*, understood in this sense, became one of Vidal's favorite analytical tools, and subsequently one of the most widely used concepts in the classical period of French Geography.<sup>4</sup>

Should we call these French descriptions objective or impressionistic? Done in the manner of the master they were both. Vidal had a research paradigm in which this blend of objectivity and impressionism functioned to organize data around this concept. Thus the critics who say his was description for its own sake were both right and wrong in different senses. Here is an appropriate point at which to suggest that one can only properly view the task of description within the context of the particular paradigm of research adopted.

#### Quantitative Description in Geography

If we accept the changing research paradigm in geography outlined in Chapter 2 (the reversion to a nomothetic approach in the 1950's in preference to the more idiosyncratic forms of studies of earlier decades) then new functions are found for certain types of quantitative methods of description. Quantitative techniques were used previously within the existing methodological framework (i.e., essentially toward idiosyncratic ends), but it was only when these techniques were used in conjunction with a new paradigm (an essentially nomothetic one) that concern over quantification became more widespread. As long as the application was limited to essentially descriptive statistics, few individuals felt upset. On this point, it should be remembered that some of the first methodological debates concerning the application of statistical



methods in geographic research<sup>5,6</sup> were concerned to a large extent with problems of statistical inference. The significance of this observation is that the function of inferential statistics is to provide tests of hypotheses, to allow for the ascertaining of levels of significance within a problem solving context. In short, they were being used as part of a more rigorous scientific methodology which by its very nature posed a dilemma for a discipline organized along idiographic lines.

Inevitably, the increased rigor provided by mathematization of concepts made apparent the weakness of the theoretical constructs it was intended to clarify. Hence the change which is evident in more recent methodological writings from arguments for more and better mathematical techniques toward appeals for more and better theory<sup>7</sup>.

This is perhaps the most important function that mathematics has performed for the discipline. Geographers now appreciate much more fully the reciprocal nature of the relationship between systematic empirical studies and systematic conceptual elaboration. This has encouraged a much more insightful application of quantitative methods. Burton recognized the nature of this relationship when he wrote that:<sup>8</sup>

The need to develop theory precedes the quantitative revolution, but quantification adds point to the need, and offers a technique whereby theory may be developed and improved. It is not certain that the early quantifiers were consciously motivated to develop theory, but it is now clear to geographers that quantification is inextricably intertwined with theory.

### Definition and Measurement

The role that quantification has played in the development of the discipline has undergone considerable change and evolution. One of its earliest functions was to highlight the need for more precise definition and more accurate measurement.

Peter Caws<sup>9</sup> as quoted in Ackoff<sup>10</sup> discusses the relationship between definition and measurement:

Definition, in general, is concerned with the systematic order of the conceptual scheme of science, and with the nature of the relations between entities. Measurement has a more limited function, that of establishing metrical order among different manifestations of particular properties, and of making scientific events amenable to mathematical description.

The nature of definition in geography, like that for any other science, is dependent upon the purposes of the researcher. A major problem arises from the attempt to quantify concepts which are ill-defined. That is not to say that ambiguous concepts cannot be given quantitative expression, but the usefulness of such exercises for the development of theory is clearly questionable. Ackoff relates an amusing incident in which he attempted to ascertain the accuracy of a survey which was intended to provide data on the number of rooms per dwelling unit.<sup>11</sup>

The survey had been conducted without an explicit definition of "room". He met the designers of the survey and asked what definition they had used implicitly. They were impatient with the question, observing, "Everyone knows what a room is". The author persisted, and one of those present offered: "A room is a space enclosed by four walls, a floor, and a ceiling". The conversation then proceeded much as follows:

The author asked, "Can't a room be triangular" ?  
"Sure. It can have three or four walls".  
"What about a circular room" ?  
"Well, it can have one or more walls".  
"What about a paper carton" ?  
"A room has to be large enough for human occupancy".  
"What about a closet" ?  
"It must be used for normal living purposes".  
"What are 'normal living purposes'" ?  
"Look, we don't have to go through this nonsense; our results are good enough for our purposes".  
"What were your purposes" ?  
"To get an index of living conditions by finding the number of persons per room in dwelling units".  
"Doesn't the size of the room matter" ?  
"Yes, we probably should have used 'square feet' of floor space, but that would have been hard to get".  
"Doesn't the height of the room matter" ?  
"I guess so. Ideally, we should have used volume".  
"Would a room with 10 square feet of floor area and 60 feet high be the same as one with 60 square feet of floor area and 10 feet high" ?  
"Look, the index is good enough for the people who use it".  
"What do they use it for" ?  
"I'm not sure, but we've had no complaints".

Just as the function of description in a research paradigm suggests the most appropriate mode of description, so the purpose of the researcher is central to producing adequate definitions, which in turn depend upon the theoretical framework employed. Measurement too is best viewed in terms of the functions it is meant to perform in a research design.

Measurement "is not in itself desirable or undesirable. It is only useful or not useful. It has, however, a number of important functions in science and, as with other forms of observation, measurement can be judged only by asking how well it performs the particular function required of it".<sup>17</sup>

The functions of measurement, elaborated by Harvey, include (i) simple

standardization, (ii) search for greater degree of precision, and (iii) symbolization. Each of these functions of measurement is useful in the task of geographical description. Standardization, for example, provides the basis for comparison of regional attributes which might be either tedious or impossible otherwise. Characteristics of places and things can be more definitively and conveniently discussed. So too the use of quantitative measurements adds precision to geographical description. Terms such as temperate or humid can be expressed in terms of numerical limits, etc. And of course symbolization, the "coordination of a particular set of events or attributes to a symbolic system", has found application in cartography as well as other areas of geographic description.

There are in fact a number of alternate systems of measurement, each with its own particular properties and range of functions and each has found application in geographic research. The most general of these systems include (i) nominal scaling, (ii) ordinal scaling, (iii) interval and ratio scaling. As the properties of these systems are generally well known and have been the subject of considerable attention elsewhere,<sup>13, 14, 15, 16</sup> it is probably not necessary to elaborate upon them here. Suffice it to say that each of these systems has been used in geographic research, although the last category is used more frequently because of its greater flexibility.

In the development of modern geography one can observe a continual change in the types of methods and languages used in seeking explanations. This progress of mathematization of the discipline has been treated explicitly in Chapter I. Such evolution has been crucial in the development of almost

every science. Hempel has commented on this point:<sup>17</sup>

In the initial stage of scientific inquiry, descriptions as well as generalizations are stated in the vocabulary of every day language. The growth of a scientific discipline, however, always brings with it the development of a system of specialized, more or less abstract, concepts and a corresponding technical terminology.

The increasing reliance upon quantitative modes of expression in geography can thus be seen as a natural part of the development of the discipline as more adequate theoretical concepts are articulated. This again serves to illustrate the point made in earlier chapters that the development of quantification and the growth of a theoretical orientation in the discipline are highly related. There is a certain reciprocity in the growth of these two elements of modern geography which suggests that substantial growth in the one is dependent to a certain extent on the growth of the other. Such change might be illustrated by reference to the function of classificatory concepts in research.

Classification has held an important place in geographic research, even if at times it was not entirely clear what function it was meant to perform.<sup>18, 19</sup> The relationship between the technical and methodological aspects of classification and the alternate uses of classification methods in research activities are discussed at length in the following chapter and need not be elaborated on at this point. Suffice to say that in many cases the use of classificatory concepts permitted the organization of observations into some convenient, and at times meaningful, order which formed the basis for descriptions and/or generalizations. In this regard, the classificatory concept functions to separate observations on the basis of some characteristic or attribute of interest in the investigation. As Hempel has observed:<sup>20</sup>

Science uses concepts of this kind largely, though not exclusively, for the description of observational findings and for the formulation of initial, and often crude, empirical generalizations. But with growing emphasis on a more subtle and theoretically fruitful conceptual apparatus, classificatory concepts tend to be replaced by other types, which make it possible to deal with characteristics capable of gradations.

The last point made suggests that the discipline increasingly began to employ concepts involving considerations of "more or less" in place of the "either .... or" type concepts associated with classification.

The increased use of non-classificatory concepts in research results from several distinct advantages of these concepts (after Hempel):<sup>21</sup>

- (a) "By means of ordering or metrical concepts, it is often possible to differentiate among instances which are lumped together in a given classification; in this sense a system of quantitative terms provides a greater degree of flexibility and subtlety".
- (b) "A characterization of several items by means of a quantitative concept shows their relative position in the order represented by that concept".
- (c) "Greater descriptive flexibility also makes for greater flexibility in the formulation of general laws".
- (d) "The introduction of metrical terms makes possible an extensive application of the concepts and theories of higher mathematics: General laws can be expressed in the form of functional relationships between different quantities."

It would appear from observing the progress of quantification in geography that in the early stages of its introduction the first two advantages were more clearly recognized and that the latter two advantages were only fully appreciated once the need for theory was more widely felt within the

discipline. Once it became evident that geography was in need of an articulated body of theory the advantages of mathematization encouraged efforts in that direction. For the most part the discipline has only recently embarked on that endeavor and it is therefore not surprising that at present the discipline can boast of neither a solid formal theoretical basis nor striking progress in mathematization. As argued in Chapter I, these are clearly the dual tasks of the discipline in the future.

#### Quantitative Methods for Describing Spatial Structure

In any discipline, certain objects or attributes come to hold an almost intrinsic value. Within geography, a number of researchers have been concerned with providing adequate descriptions of certain features of spatial distributions through the application of quantitative techniques. Aside from the use of numbers, and to a certain extent arithmetic operations (which for the most part we will not discuss), in statements such as "the average yield per acre is..." or "production in region A is twice that in region B...", the largest use of quantitative descriptive statements by geographers has been in the attempt to provide concise, and at times more precise, measures of certain properties of spatial distributions.

For the most part, it has been convenient to generalize distributions to some extent so that one can define the elements as points, lines or areas. Characterizing (i.e. describing) spatial distributions consisting of points, line, or area elements has been the focus of a considerable amount of work in recent years. Attempts were made to provide convenient descriptions of spatial distributions, even when the characteristics thus elaborated were not

of any known theoretical significance. At an early period in the quantitative revolution, and in fact predating that revolution, there was a concerted effort by geographers and others interested in spatial analysis to provide descriptions of spatial or geographical series (i.e. spatial statistics) comparable to those which existed for statistical series. The work of the centrographers in this regard is of major significance. The work of these early quantifiers, as well as the more recent work by R. Bachi<sup>22</sup> and D. Neft<sup>23</sup> provided the discipline with a large number of quantitative descriptive measures of central tendency (areal mean, areal median, etc.) and dispersion (standard distance) in geographical series. Perhaps one of the best examples of the use of quantitative descriptive measures of this type is the calculation of population centers. Similarly, measures of dispersion and concentration in locational patterns have been developed to provide concise descriptions which are felt to be particularly revealing of "the nature of things".

The introduction of quantitative descriptive measures in geographic research served two related purposes. In some cases the use of quantitative measures was viewed as an attempt to provide more "objective", precise, and parsimonious descriptions of features of interest. In this sense the introduction of quantitative methods of description was of limited import in the growth of a more theoretical geography. A second, and methodologically more important, function was the linking of these quantitative descriptive measures with conceptual models and theoretical statements. In the latter case the quantitative descriptive measures were used to test hypotheses developed from conceptual models of the phenomenon or process of interest.



The distinguishing features, then, was not the application of this or that quantitative method, but rather the use to which it was put in the research design. The same quantitative technique could equally serve either end. The important development for the discipline was the recognition of the need for increasing use of the second type of function. As the introduction of quantitative methods proceeded in the discipline, there was an initial interest in the first use (i.e., being more objective) whereas more recently, the second function has been recognized as the more important. An illustration of these two functions and the change in viewing their role in research may be provided by noting the use of a few techniques of quantitatively describing spatial distributions in the analysis of settlement patterns.

A considerable amount of research in urban geography has concentrated on the description of settlement patterns. Given a map of town locations, represented by a point distribution, numerous attempts have been made to characterize the pattern exhibited. In attempting to describe the settlement pattern a number of methods are available. Much of the earlier work depended upon verbal, qualitative assessments of the pattern. In an early attempt to provide more precise and quantitative methods of describing settlement patterns, King argued that "...the greater part of the descriptive work to date has been characterized by an almost unquestioning reliance upon terms as "sparse", "dispersed", "agglomerated", or "dense". While these terms have meanings with reference to the context in which they are used, they lack objectivity for more extended comparative purposes. It appears desirable, therefore, that a more precise connotation be given to these descriptive terms as a means

of facilitating comparative analysis and the discovery of more universal generalizations concerning the nature of settlement patterns."<sup>24</sup> Note that the main argument presented in this statement is for the use of quantitative measures as providing more precise and objectives measures of the characteristics of interest; reference to their role in the development of theoretical geography is made only in a restricted sense. In this context several quantitative techniques are available for describing attributes of spatial distributions. One such characteristic would be some measure of central tendency in the distribution as represented by, say, the areal mean. Similarly, calculating the standard distance of the distribution would provide a measure of dispersion in the settlement pattern. Each of these simple descriptive devices would be useful in comparing the distribution with others. They each describe features of the distribution which are of intrinsic value, even though unrelated to any conceptual model or theory. In this sense their use is strictly descriptive. However, it is also possible that such measures may be used more explicitly toward theoretical ends. To illustrate, let us measure the distances between each point (settlement) and the point nearest to it (i.e., its nearest neighbor). Summing these and dividing by the number of points results in the mean near-neighbor distance,  $\bar{r} = \sum r/N$ . Again, this represents one simple descriptive quantitative expression of an attribute of geographic interest and as such it could be compared to others, etc. But the same expression could be used in conjunction with a theoretical concept to explicitly test a research hypothesis. If the mean near-neighbor distance for a theoretical distribution is known the empirically calculated

value may be compared to it thus providing a test of whether empirical and theoretical values are the same or similar. One such measure discussed by King, which depends upon the Poisson distribution, is the ratio between observed and theoretical distances (based upon the Poisson) termed the near-neighbor statistic ( $R = \bar{r}_o / \bar{r}_p$ ). This statistic ranges from 0 to 2.15 and allows for the comparing of an observed distribution with a theoretical random distribution. In this case the theoretical random distribution is used as a null hypothesis, i.e., that the pattern exhibits no discernible order. Here the use of the quantitative technique goes beyond simple description to comparison with a theoretical model. However, even this procedure may be of limited value theoretically since it allows only incomplete (subjective?) specification of the nature of the process operating to produce the settlement pattern (e.g., more regular than random). In tests such as these the geographer hopes to reject the null hypothesis of no discernible order since his science is concerned with discerning and explaining spatial order by reference to spatial process. But this type of technique has often been used in a rather limited fashion. For while several studies have utilized near-neighbor methods to assess randomness in spatial distribution, few provide explicit models to account for non-randomness. Clearly it is not enough to simply imply that order exists, but rather it is necessary to provide sound theoretical statements of the process operating. A recent paper by [unclear] points out the need to go beyond simple quantitative description and recognizes the need for quantitative methods to function within a theoretical context. He argues that "...statistical technique is no substitute for theory.

The usefulness of theory and predictive models in geography is now a matter of record. Theories of location explain the laws of spatial distributions. Unless geographical explanations (or predictions) have a theoretical justification, the recognition of spatial regularities is of little value."<sup>25</sup>

It is probably safe to say that a considerable proportion of the attempts to employ quantitative methods has aimed at providing descriptions of attributes and relationships of interest to the geographer. Despite the increasing use of quantitative techniques, much of the work done is still basically descriptive in nature. In some instances, however, the function of such descriptions is markedly different than the earlier attempts at description in the discipline. Description is not limited to "conveying an impression of what the landscape looks like", but rather serves as a basis for subsequent scientific analysis.

Much of the work using quantitative descriptive measures has concentration on the description of what has come to be termed "spatial structure". While this term has found widespread use among geographers, it has yet to be adequately defined. As noted in the discussion of epistemology in the last chapter, it appears that just what is meant by spatial structure depends upon the particular context in which it is used. In one such context it has come to be associated with the notion of the manner in which objects or events are organized in geographic space. So that when used, it generally is meant to define a system of relationships (in this case spatial) existing between entities which is ordered and discernible.

Most of the work on this type of quantitative description has been concentrated on the description of point patterns. Dacey and McConnell have provided useful reviews of these measures and their application to geographic research.<sup>26, 27</sup> A more limited body of literature has dealt with methods of describing line patterns. Dacey reports on some attempts to describe line patterns,<sup>28</sup> which although not wholly satisfactory, do suggest the types of measures which may be obtained. A more satisfactory approach to the study and description of line patterns has been through the application of graph theoretic concepts. Karsky's analysis of transportation networks provides a number of descriptive methods for describing certain features of networks.<sup>29</sup> More recently, Chorley and Haggett<sup>30</sup> have reviewed much of this literature in their book, Network Analysis in Geography. Geographers have had considerably less success with developing adequate methods of describing certain characteristics such as the shape of an area. Some of the various attempts are discussed by Bringe,<sup>31</sup> and Boyce and Clark.<sup>32</sup>

While much of this work can be classified as essentially descriptive in terms of use, it is not always possible to draw a clear distinction between those uses which are basically descriptive in nature and those which are more intricately involved with some theoretical objectives. It is safe to suggest however, that the bulk of the work using these measures is essentially descriptive.

This has also generally been true of the attempts to obtain quantitative descriptions of areal patterns or surfaces although the degree of quantitative sophistication of some of the more recent attempts has been increased. The conventional methods of cartographic analysis have been supplemented by the more analytical techniques of trend surface and Fourier analysis. The use of the trend surface methods<sup>33</sup> has gained considerable acceptance in the treatment of areal data. Most of the applications of trend surface analysis have been limited to the identification and description of regional and local trends in areal data through the fitting of increasingly higher order surfaces although the method also holds some promise for the description of directional data.<sup>34</sup> Other methods of describing variance in areally distributed data such as Fourier analysis and spectral analysis have recently been the subject of some geographical investigations.<sup>35</sup> But, despite the increased sophistication of these methods, their use has been almost entirely limited to description. Curry<sup>36</sup> recently commented on this point:

The quantitative description of the spatial distribution of any phenomenon which can be regarded as continuous has been attempted on numerous occasions. A number of different methods have been employed but while their elegance is admirable it is usually difficult to interpret the results. They are generally to be regarded as "pure description" and any analytic merit they enjoy is fortuitous. Until we have theory which allows us to anticipate the mathematical form of the surface, the fitting of arbitrarily chosen functions cannot be itself provide insights.

Even as "pure description", these techniques of quantitative measurement provided the discipline with more accurate and more parsimonious methods of describing the phenomenon of intrinsic interest. The sub-

stitution of analytic tools such as trend surface analysis for more conventional procedures such as isoplething yield considerable gains. The fact that the particular surface fitted to the areally distributed data set is specified by an explicit mathematical function means that:

- (i) The value of the surface at any reference point is determined in relation to all other data points to provide a "best fitting" surface.
- (ii) The degree of correspondence between such a generated trend surface and the original observations is measurable since the extent of the divergence between observed and predicted (surface) values is quantitatively assessed in a measure of residual variation in the data which is not "explained" by the surface.
- (iii) Increasingly complex functions may be used to "fit" the surface, while the corresponding reduction in residual variation is reflected in the residual sum of squares.
- (iv) Alternative hypothesis regarding the nature of the surfaces may be quantitatively assessed.

Their use has not, however, been strictly limited to the first conception of description. Several attempts have been made to use the output of these analyses in a manner related to theoretical concepts. Rather than as measures of interest in themselves, such quantitative descriptions have been used to test predictions derived from theoretical statements. Getis<sup>37</sup> used near-neighbor and quadrat methods to test for expected distributions of retail stores through time based upon a conceptual model of locational change in retail structure of urban areas. Hyslop<sup>38</sup> has used these same techniques to analyze changes in rural settlement patterns based upon a conceptual model positing an evolution from an

initially random pattern through a phase of colonization (clustering) and finally to a pattern based upon a process of spatial competition (regular spacing).<sup>58</sup>

Here, the quantitative descriptions are used to compare observed characteristics with those predicted by theory. Used in this fashion, the quantitative descriptions are seen to be important not in terms of their intrinsic value but rather as comparisons with some expected value. Partly owing to the generally sparse nature of theoretical studies in geographic literature, examples of this type of use are somewhat limited although this is clearly the primary use to which such techniques will be used in the future.

#### Quantitative Description and Inference

Even when primarily used for purposes of simple description, it is often implicitly understood that the function of quantitative descriptions is to provide the basis for deriving certain inferences about the phenomenon of interest or the process which has resulted in the particular spatial structure being investigated. That is to say, from a particular quantitative description or measure one wishes to be able to infer certain other features or relationships of relevance to the problem at hand. In order to insure that the inferences drawn are correct, it is necessary to insure that such inferences are logically derived. But quite apart from insuring that the inferences are logical, it is first of all necessary to be convinced that the particular measure (i.e., description) itself is valid.



This validity is itself related to the particular function that the measure is to perform. Harvey suggests that it is "impossible to assess the validity of a given measure without considering its purpose".<sup>39</sup>

That is, having succeeded in describing some characteristic or attribute by any of the quantitative methods discussed above, on what basis can we consider its validity. Harvey, following Nunally,<sup>40</sup> discusses:

(i) Predictive validity, (ii) Content validity, and (iii) Construct validity.

When the purpose of the particular measure is to predict or estimate some other characteristic or feature, its predictive validity really depends upon how well it performs. In this sense, it is not too difficult to assess the performance of a particular measure; either it serves as a useful predictor or it does not. The other types of validity for measures are somewhat more difficult to assess. For example, the content validity of a measure cannot be assessed with respect to its correlation to some other value. As it is conceived as a measure of some particular aspect or attribute of the phenomena itself, we cannot validate it by reference to other attributes. Rather, we must scrutinize our procedure in arriving at the measure to insure that it is logically sound and mathematically accurate. As Harvey notes, this amounts to a "kind of mini-experimental-design procedure as a precursor to measurement". In effect we are attempting to insure that we are indeed measuring that which we wish to measure. "The aim of such validation procedure is to ensure that the measure relates only to the attribute being measured (i.e., there are no interfering forces) and that the attribute is measured in an

unbiased manner."<sup>41</sup> The final form of validity, construct validity, can be even more difficult to assess. The reason being that rather than evaluating a direct measure on phenomena, we are attempting to assess some characteristic which is not directly observable. Despite the fact that these measures are the most difficult to evaluate or validate, they are perhaps the most crucial form of measure since they generally are suggested by theory. Such theoretical constructs are essential to the development of an abstract geography and we therefore require considerably more effort to ascertain the most effective methods of obtaining and evaluating them. Coleman also distinguishes between these direct measures (content measures) and those which he terms "dispositional" measures.<sup>42</sup> The latter are more difficult to assess, although they are more "fundamental" in the sense that such constructs often are derived from theory and therefore promise greater insight than simple descriptive measures. Significantly, as noted in the preceding chapter, it is into the area of such construct measures that the field is currently progressing. The increasing importance of theoretical formulation in geographic research has necessitated the development of more and more methods of arriving at measures of this second, construct type. Unfortunately, the methodology of handling and evaluating such measures is not nearly so well developed nor understood as that for the more traditional content type of measure. Until greater attention is given to these problems, we will remain only partially prepared to significantly advance the level of our understanding of

geographic problems. The reason for this is obvious. Even with the most advanced methods of measurement, properly used, we are left at best with accurate descriptions. In order to effectively develop a theoretical geography, we must further be capable of transferring this descriptive knowledge to the plane of explanation. This is essentially a problem of inference in geographic research. That is to say, that measuring (even measuring well) certain properties of some spatial structure provides content data of a sort which is perhaps descriptively useful, but which in themselves are of limited value so long as the nature of the relationships or processes which account for those particular values provided by the measurement remain undefined. This is the crucial role of theory: to provide rational explanations for the values which occur or to suggest expected values against which the empirically derived values may be compared. Without such theoretical rationale, even the best of measures provide only interesting empirical facts, or where recurring, an empirical regularity for which no real explanation exists. However, as discussed in the last chapter some basic problems exist in formulating geographic theory such that its theorems are specifically spatial and thus amenable to testing by the methods discussed above.

This is perhaps best illustrated in the increasing attempts by geographers to deal effectively with those types of measure identified above as "dispositional" or construct measures. Here the difficulties of both measuring and drawing inferences are becoming

increasingly apparent. In order to go beyond the first type of basic description (whether quantitative or not) discussed earlier and to provide theoretical insight, it is necessary to begin to define and measure certain properties or attributes which are not directly observable. The increasing interest in certain behavioral-type studies provides numerous examples of the sort of measures under discussion. The growing body of literature dealing with these "behavioral" aspects of geography abound with terms such as "action space", "place utility", "perceived distance", etc.<sup>43</sup> In certain of these studies it would appear the the ability to "measure" certain properties has outpaced theoretical conceptualization. We are not here concerned with whether in fact these attempts at measuring such attributes are sound. (For an interesting discussion of the problems of measurement in studies of the space-perception variety, see Harvey).<sup>44</sup> The point is that without adequate theoretical rationale, such "measures" are likely to be useful only in a limited, descriptive sense, despite the application of quite sophisticated quantitative techniques. Many of the attempts to study perception of the "mental maps" variety are almost wholly descriptive in character and often are not as meaningful as a simple map of migration streams. The necessary theoretical work is only now being taken up at all seriously. Attempts such as Rushton's recent studies of preference structures begin to provide insight into the role of perceptual processes at the behavioral level in a way in which the earlier descriptive studies could not possibly contribute.<sup>45</sup> One obvious difficulty for the researcher

working in this area is that the often methodical, and somewhat slow, progress in this approach is less attractive than the "grand over-view", and has less immediate impact than the broad generalizations even though methodologically they are considerably more sound.

#### A Conceptual Basis for Geographic Description

Nystuen realized at a quite early stage the need to provide a sound basis for the development and use of geographic or spatial constructs. Noting that many of the frequently recurring terms in the geographic literature such as distance, relative position, accessibility, spatial pattern, etc. seemed all to depend upon certain basic concepts. Nystuen asked "What subset of these common words are necessary and sufficient to employ the geographical point of view?".<sup>46</sup> In his discussion of the terms which he felt were a necessary basis for the geographic viewpoint, Nystuen included the concepts of (i) directional orientation, (ii) distance, and (iii) connectiveness or relative position. Several years later Papageorgiou suggested that "..... the word point must be added to the existing set because ... it is necessary for the description of spatial systems and because its meaning cannot be derived through the meaning of the words in the basis".<sup>47</sup> Although both authors attempted to define concepts which were necessary to the formation of a basis for geographic analysis, neither claimed to have defined a set which was sufficient for that basis. Both, then, suggested that additions to the basis will probably be necessary. Whatever the final outcome of such discussions, it is

clear that the terms presently accepted as forming part of the basis have been the subjects of numerous geographic studies. Many of these studies dealt with the problems of operationalizing these concepts and much of the initial quantitative literature was directed toward providing means of measuring them.

We have already discussed certain features of the work which has been undertaken in the analysis of point patterns. The other elements in the basis described by Nystuen and Papageorgiou also have interested geographers for a long time. Perhaps, the element of distance has received the most attention having figured in geographic research in a number of ways. This interest in distance is what prompted Watson to term the field a "discipline in distance".<sup>48</sup> One basic research area associated with the analysis and use of this important concept is the problem of definition and measurement. It was early recognized that distance may in fact be interpreted in a variety of manners. Distance has been seen to be a concept which may be operationalized in a variety of metrics, the particular metric which is appropriate depending upon the type of problem under consideration. Thus distance has been used in geographic studies in a number of ways and the problem of effecting transformations of distance has itself given rise to an interesting literature.<sup>49, 50</sup> In its various operationalized forms, distance has been a central variable in many geographic studies. In human geography it has

held a prominent position in most studies dealing with spatial interaction of whatever type. Much of the literature using distance in the study of human interaction has been reviewed by Olsson<sup>51</sup> and we need not discuss it at length. Harvey, has also recently discussed certain of the measurement problems associated with the use of the distance concept.<sup>52</sup>

What Nystuen termed connectiveness, or relative position, also has featured in a large number of geographic studies. Attempts have been made to operationalize the term in a number of ways. In some cases the purpose of such studies was simply to provide a way of measuring this attribute -- essentially a descriptive end. In others, the initial description was to form the basis for drawing certain inferences or comparing the results with those suggested by some theoretical considerations. For example, the efforts of Garrison, Marble and some of their colleagues at Northwestern during the early 1960's were directed toward establishing useful measures of such properties as connectivity, accessibility, etc., all of which are based upon the concept of relative position. The work on the application of graph theoretic measures to geographic research which has come out of that effort has provided the discipline with a number of interesting and useful methods for describing spatial patterns in terms of this basic concept.<sup>53</sup> Employing such quantitative measures, others have attempted to relate the descriptions thus arrived at in testing certain theoretical notions. This is perhaps best illustrated with regard to the

concept of centrality as articulated in the Central Place Theory of Christaller. The analytic methods provided the means for describing quantitatively the attributes of certain towns in terms of their centrality to an urban system. These in turn were used to identify certain hierarchical features of the urban system<sup>54</sup> or to predict certain other features suggested by central place concepts. So, for example, Gauthier has relied on these descriptive devices to identify the degree of centrality of towns in Brazil and these measures are in turn related to other features of the urban centers, e. g. level of development, etc.<sup>55</sup>

Both of the concepts discussed thus far have received greater attention than the third--directional orientation. For the most part, this concept has not been investigated nearly to the extent that others in the basis have. An example, one can cite the numerous studies of migration that have been undertaken in geography. In nearly every case, the authors have attempted to discuss the effect of distance upon the volume, composition, etc. of migrants. Any directional bias in migration streams is often afforded only peripheral attention. There have so far been relatively few attempts to deal with directional orientation in an explicit fashion. Wolpert<sup>56</sup> has used simple descriptive statistics of central tendency to portray the directional bias in certain migration streams for major metropolitan areas of the U. S., and more recently Adams<sup>57</sup> has attempted to analyze directional bias in the pattern of intra-city migration. Some aspects of these problems as they relate to migration streams were recently discussed



by McNulty<sup>58</sup> in a paper aimed at providing an example of the use of trend surface techniques in the description of directional components of migration streams.

It may be seen that with regard to each of the concepts discussed, geographers have attempted to provide accurate measures and to use these measures for the purposes of description and explanation. The use of these measures for essentially descriptive ends, however, is far more common than attempts to relate them to some theoretical structure. This primarily results from the general paucity of articulated geographic theory.

#### Quantitative Description and Theory Construction

We have had occasion, in several chapters of this work, to discuss the interrelated nature of theory formulation and mathematization of the discipline. One might well question this recurring argument and ask the question recently posed by Coleman:<sup>59</sup>

What is it about quantitative measurement that is so crucial for theory development? Essentially this: the power of a theory to provide precise and numerous deductions lies in its ability to carry out transformations in fact, chains of transformations -- upon the input data.

If these data are in the form of numbers, and maintain their properties as numbers after the transformations, then the powerful transformations of algebra, calculus, and matrix algebra can be carried out upon them.

In many instances in geography, as discussed in Chapter I, the powerful calculi provided by mathematics was not utilized to the greatest extent possible. Many early uses of quantitative methods were limited to arithmetic operations necessary for the comparison or description of geographic

phenomena. It is true, as Coleman has noted, that there "has never been such a simple correspondence between mathematical structures of relation between elements in social science" as was true, for example, between the relationships studied in the physical sciences (as in the study of mechanics) and the structure of algebra. Coleman suggests that "one of the reasons has been that no generally useful and easily measurable set of elements (or 'concepts') has been posited in most of social science". However, he noted that "In those few areas where such concepts have been specified, particularly in economics, the use of mathematics has flowered".<sup>60</sup>

One of the major functions performed by quantification of the discipline, then, has been to identify areas of weakness within the conceptual apparatus of the field. In the future, one can expect an increasing reliance upon quantitative methods used within a conceptual or theoretical framework of analysis. This recognition of the need for a more theoretical orientation to research should result both in better use of existing quantitative techniques as well as an increase in the number of theoretical models available to geographers.

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CHAPTER V

REGIONALIZATION AND CLASSIFICATION:  
AN INTERPRETATION AND EVALUATION OF RECENT  
TRENDS TOWARDS QUANTIFICATION

Geographers have been traditionally concerned with regions and systems of regions. The rise of a 'quantitative geography' has not led to a decline in this interest but has rather accompanied it in a change of emphasis. The regional concept no longer serves as a means of investigation in itself but has taken up a role analogous to that of the class in other taxonomic study. This classification-regionalization analogy has been most fully drawn in two papers by David Grigg.<sup>1</sup> However, in reviewing the second paper by Grigg, Brian Berry<sup>2</sup> has commented:

"A fine statement on regions and classificatory processes is slim in its treatment of the concept and processes of modern numerical taxonomy."

This is in spite of the fact that Grigg uses the parallel development of numerical techniques in biological and regional taxonomy as part of his analogy and it would seem that it is these quantification trends that have brought the analogy to the forefront. Grigg claims to be concerned with "strategy rather than tactics" the latter being covered in the substantive work of Berry and the recent review by Spence and Taylor.<sup>3</sup> However, Grigg's relative neglect of quantitative classification precludes his discussion of strategy from covering the implications of numerical regionalizing for geography, in particular the relationship between regionalization and the

overall strategy of developing a theoretical geography. This chapter of the report attempts to fulfill this rather ambitious role. The result is an essay that explores recent taxonomic thought in an attempt to find other than strictly empirical answers to the question 'why classify?' or 'why regionalize?'.

### Taxonomic Practice

One of the peculiarities of classification is that it has been dominated by the taxonomy of animals and plants. In fact, the term 'taxonomy' with no prefix is commonly restricted to the study of classifying animals and plants. Even within biology other classification, such as in plant ecology, is omitted from 'taxonomy proper'. In this essay prefixes will generally be used when a particular set of classifications are referred to so that, in the above case, the former becomes biological taxonomy and the latter ecological taxonomy. It is worthwhile considering this dominance of biological taxonomy a little further since it is later argued to have detrimental effects on taxonomy as a whole.

#### 1. The dominance of biology.

The present restricted use of the term taxonomy does not always seem to have applied to practitioners of classification. Thus Aristotle produced classifications in chemistry as well as biology as did the famous eighteenth century taxonomist Linnaeus.<sup>4</sup> However, in the post-Darwin era biological taxonomists have tended to become somewhat inward looking so that Gilmour<sup>5</sup> in particular has long denounced biological taxonomy's isolation particularly with respect to philosophy and logic. In contrast many other disciplines have



looked to biological taxonomy as a model classification system. This probably stems from the generally high reputation of evolution theory which led to many imitations after 1859. The result has been a whole series of genetic classifications. In the social sciences the Marxist's stages in the evolution of society both parallels, and follows on from, Darwin's theory. Gilmour and Walters<sup>6</sup> mention Clement's developmental classification of vegetation leading to a climax stage and the Russian school of genetic soil classification. In geography the Davisian categorization of the physical landscape represents another genetic approach.<sup>7</sup> Even in more developed sciences, such as chemistry, classifications have been based on direct analogy with biological taxonomy and have related to theories of the origins of chemical elements.<sup>8</sup> An even more obtuse example is the attempt to produce a genetic approach for categorizing knowledge in library classifications.<sup>9</sup> Thus biological taxonomy has, with just one or two exceptions such as psychology, assumed the role of the basic paradigm for classification studies.

In drawing the classification-regionalizing analogy both Bunge and Grigg<sup>10</sup> draw heavily on biological examples and this situation has now extended into the quantitative era of taxonomy with Sokal and Sneath's textbook on numerical taxonomy<sup>11</sup> assuming the role as a basic text for quantitative classification in many fields.<sup>12</sup> However by no means all classification has waited for biological taxonomy to quantify.

Trends in quantification: biological and psychological classification.

For the purposes of this essay 'quantification' is defined as not only

the use of quantitative measurements but further the application of numerical techniques on these measurements. Given this restricted definition we can identify two disciplines that seem to differ greatly in the application of a quantitative approach to classification - on the one hand biology has experienced a revolution while psychology has gradually evolved a set of numerical classificatory procedures.

The biological sciences have instigated and contributed to the development of modern statistical analysis from Galton through to Fisher. However, the application of numerical techniques to classification has varied greatly within biology. On the one hand Fisher developed discriminant analysis in the 1930's<sup>13</sup> while at the same time the new systematics of Huxley<sup>14</sup> was largely unconcerned with numerical procedures. Thus Fisher's work found application in physical anthropology and the stratification of samples in statistics but not in biological taxonomy itself. Similarly in the 1940's and early 1950's plant ecologists were developing grouping strategies<sup>15</sup> and divisive procedures<sup>16</sup> with little or no feed back into biological taxonomy. Thus the development of numerical classification of the animals and plants themselves had to wait until the second half of the 1950's. In 1957 papers were published by both Sokal<sup>17</sup> and Sneath<sup>18</sup> which proposed new grouping algorithms and this touched off what Simpson<sup>19</sup> has termed an 'explosion' of numerically based publications in taxonomy. One result has been the newsletter Taxometrics whose bibliographic sections, brought together by Maisel,<sup>20</sup> give graphic evidence of the extent of this quantitative revolution in biological taxonomy. The most interesting feature of this trend was that it occurred

several decades after most other biological research had accepted quantification.

Francis Galton, as well as contributing to the Biometrics school, can also be identified as an early instigator of the psychometrics school. The work of Galton on mathematical approaches to classification and the identification of types led directly to Pearson's introduction of factor analysis and the subsequent development of the technique by psychologists.<sup>21</sup> Directly associated with this work, some factor analysts also produced more simple algorithms such as Cattell's ramifying linkage method<sup>22</sup> and Holzinger's clustering technique using the 'coefficient of belonging'.<sup>23</sup> Thus in contrast to biological taxonomy, developments in numerical classification in behavioral taxonomy seem to have been an integral part of the broader development of psychometrics since 1900.

## 2. Quantification in regional taxonomy.

The question now arises as to whether regional taxonomy has experienced trends similar to either biology or psychology. Grigg recognizes three stages in "the sophistication of regionalization":

"First was the recognition that regions should be delimited upon the basis of properties of the individuals regionalized, and not upon the basis of some supposed 'cause' of the regions. Second, was the distinction made between uniform and nodal regions, and between generic and specific regions. The third vital step in progress has been the use, both of descriptive statistics in the establishment of regional systems, and more recently of analytical statistical methods - particularly factor analysis - which have brought a greater rigour to the delimitation of regions."<sup>24</sup>

It is this final stage that is of interest here. Simple descriptive statistical procedures were being used by the 1930's and these consisted of two major

approaches. The first was the isoline method whereby regional boundaries were identified from critical isolines. This technique was developed by Jones<sup>25</sup> and used by Hartshorne and Dicken<sup>26</sup> "to put classification on a scientific i.e., measurement basis." The alternative procedure was to compute 'indices' from several variables so that when mapped they indicated the regional pattern of an area. These early attempts at multi-variate analysis are described by Renner<sup>27</sup> and would seem to naturally lead on to the application of factor analysis to regional description. However, this development had to wait for another generation of geographers despite the fact that statisticians<sup>28</sup> and sociologists<sup>29</sup> presented early examples of the use of factor analysis with areal data.

More sophisticated statistical procedures were not introduced to regional taxonomy by geographers until the late 1950's. At this time Zoller<sup>30</sup> was experimenting with the application of inferential statistics to regional analysis and Berry<sup>31</sup> was beginning to develop his three stage taxonomic procedure involving factor analysis, distance scaling and step-wise grouping.<sup>32</sup> However, with this new development of numerical regionalizing, there has rapidly developed a very large literature on this subject in geography which, as Grigg<sup>33</sup> has noted, seems to parallel the quantitative revolution in biological taxonomy. However, unlike biological taxonomy, quantification in regionalization has not lagged behind the overall development of a quantitative geography but has been an integral part of this development. Thus the similarity with biological taxonomy would seem to be superficial and regionalization's quantitative development more closely parallels similar developments

in psychological classification. The methodological comparisons that follow confirm this conclusion.

#### Methodological backgrounds to quantification

The rapid rise of quantification in biological taxonomy since the late 1950's has often been related to the recent widespread availability of electronic computer assistance for researchers.<sup>34</sup> However, the fact that numerical classification emerged in other fields in the pre-computer era suggests that the differing methodological backgrounds between disciplines are the major determinants of the quantification trends.<sup>35</sup> This section therefore is concerned with these methodology backgrounds.

The behavioral taxonomy reviewed above belongs to the multi-variate school of psychology as opposed to the bivariate school of the Wundt-Helmoltz-Pavlov tradition.<sup>36</sup> Galton has been identified as the first researcher to explicitly recognize that simple bivariate analysis was inadequate for the development of psychology.<sup>37</sup> This school of thought, largely represented by factor analysts, has gradually developed until today it is represented in psychology by the multi-variate behavioral research group and has diffused beyond psychology into many disciplines including geography. This rather smooth development contrasts with the more interesting variations in biology which has produced the anomolous time-lag of quantification in biological taxonomy. Thus this section concentrates largely on the methodological background leading up to Sokal and Sneath's 'numerical taxonomy' before the case of regionalization and regional geography is considered.

1. The rise of empiricism in biological taxonomy.

Heywood<sup>38</sup> identifies Sokal and Sneath's 'numerical taxonomy' as the third revolution in modern taxonomy. The two other revolutions are associated with Darwin's Origin of Species in 1859 and Huxley's New Systematics in 1940. However, both of these earlier revolutions made little difference to the 'practice' of classification but related rather to the interpretation of results. The point that is important in this context is that from prior to the Origin right through to the new systematics, hypotheses have been confused with the purely descriptive role of classification.<sup>39</sup> Thus pre-evolutionary taxonomy was concerned with affinity between organism not simply as resemblance but as reflecting some 'creator's plan'. Evolution theory then simply gave new interpretation to the concept of affinity. The mixing of phylogenetic hypotheses with the practice of classification involves making value judgments by the taxonomist about which attributes are important in evolutionary terms so that the "experience" of the taxonomist is emphasized in carrying out his "art".<sup>40</sup> Such a mode of thought has two results. First, the practitioners, in concentrating on their own theoretical problems, become isolated from the concepts and methods of classification in general.<sup>41</sup> This development has already been noted above. Secondly, this situation is obviously not conducive to developments of quantification.

However one school of taxonomists have long deplored the mixing of phylogenetic considerations with classification.<sup>42</sup> At the most practical level the argument of this school is simply that "the available fossil evidence is so fragmentary that the phylogeny of the vast majority of taxa is unknown"<sup>43</sup>

and therefore any speculations concerning evolution should be omitted from the classification process. The problem in the past has been that although these criticisms have seemed to many to be quite valid, the critics have been unable to offer any viable alternative to the evolutionary school.<sup>44</sup> This is where Heywood sees the numerical taxonomists fitting into the picture. Sokal and Sneath's work belongs to this phenetic non-evolutionary based taxonomic school. Thus in this quantitative revolution Heywood looks forward to a truly 'taxonomic' revolution<sup>45</sup> as opposed to simply changes in interpretation. Note that the numerical taxonomists are not criticizing evolution theory or even querying that natural selection will usually be the reason for phenetic resemblance between organisms. Rather they require that such considerations be divorced from the descriptive classification process although such hypotheses and their testing might constitute subsequent stages of a research design.<sup>46</sup>

Thus "the argument here centers on when, not on whether, phylogenetic deductions are to be made."<sup>47</sup> With classification categorized simply as description, this approach is obviously more conducive to quantification and so numerical taxonomy has resulted.

The time-lag in quantification in biological taxonomy would therefore seem to be due to the classification process having to wait for a widely accepted methodological liberation from the theoretical overtones of the tradition of taxonomists. Thus the basis of the new numerical taxonomy school has been the rise of a purely empirical methodology. Since this approach lies at the heart of much of the subsequent discussion, Sokal and Sneath's methodological

position is described here in some detail.

Sokal and Sneath have defined 'numerical taxonomy' simply as the "quantification of the classificatory process"<sup>48</sup> or more fully "the numerical evaluation of the affinity or similarity between taxonomic units and the ordering of these units into taxa on the basis of their affinities."<sup>49</sup> These definitions are general enough to cover all quantitative taxonomy and the term has been introduced into geography on this understanding by Berry.<sup>50</sup> However, Sokal and Sneath go on to point out that "numerical taxonomy is based on the ideas put forward by Adanson."<sup>51</sup> Adanson was a contemporary of Linneaus and produced an empirically based classification of plants in 1767. Sokal and Sneath claim to be following in the "Adansonian tradition" and their work has been called Adansonian taxonomy or perhaps more accurately neo-Adansonian taxonomy. Adanson's influence is embodied in the six 'basic positions' or axioms. The first two are very important and are stated as:

- "(1) The ideal taxonomy is that in which the taxa have the greatest content of information and which is based on as many characters as possible.
- (2) A priori, every character is of equal weight in creating natural taxa."<sup>52</sup>

These two guidelines ensure the minimalization of the subjective element in the classification process. Thus bias in individual choice of variables is lessened by axiom 1 since many characters are to be used - Sokal and Sneath suggesting "at least sixty seem desirable." Furthermore "all kinds of character are equally desirable" and by axiom 2 each is given equal weight thus further limiting possible subjective biases. However, this latter axiom has come under criticism from other quantitative taxonomists.<sup>53</sup> Thus



Williams and Dale similarly object to a priori weighting of variables but point out that weights calculated internally "contravene only the letter and not the spirit, of the Adansonian postulates."<sup>54</sup> Sneath seems to accept this argument since he has recently written that "numerical taxonomy need not necessarily employ the principle of equal weight for every character."<sup>55</sup> However, the approach maintains the fifth Adansonian axiom that taxonomy is "a strictly empirical science."

This extreme empiricism of numerical taxonomy has been criticized by traditional taxonomists as "typology" which they distinguish from "biological classification",<sup>56</sup> the latter incorporating evolutionary hypotheses. Sokal has directly answered these critics by agreeing that Plato's "idealized types", with their metaphysical overtones, are untenable but that modern typology is a new form of typology representing "an empirical summation of the information available."<sup>57</sup>

This last quote leads on to what seems to be the logical implications of this new empirical school - namely that classification is merely a data sorting method. This argument is best represented in Sokal and Sneath's discussion of "efficiency in taxonomy."<sup>58</sup> They define efficiency in terms of time, or operationally as costs, and argue that computerized numerical taxonomy is the most efficient procedure for use in taxonomic institutions such as museums. Thus numerical taxonomy differs from traditional taxonomy not simply in terms of quantification but more fundamentally on methodological grounds with Sokal and Sneath's view of taxonomy as information storage and retrieval being diametrically opposed to Simpson's view of the "art" of

classification.

2. Regional geography and regional taxonomy.

Although it is possible to identify several schools of thought in regional studies<sup>59</sup> they basically fall into two distinct groups.<sup>60</sup> First, there is what may be termed 'the regional geography school' where the region is conceived as a method of study in itself. Examples are the 'pays' concept in French geography,<sup>61</sup> Whittlesey's compage concept<sup>62</sup> and the regional geography of the Oxford school.<sup>63</sup> In contrast the regional taxonomists accept regionalization as simply classification so that the regional description is not an end in itself. The former school emphasizes the skill of the geographer in 'synthesizing' the disparate elements of an area into a coherent regional description. The method is thus typically categorized as an art. Grigg<sup>64</sup> has drawn a parallel between the implicit environmental determinism of this approach and the phylogenetic basis of traditional biological taxonomy. Certainly neither is particularly conducive to the development of quantitative methods. However, the analogy cannot be drawn too far since the taxonomic school was well founded in geography by the 1920's and 1930's when simple descriptive statistics were beginning to be used but many years before the recent quantitative revolution. Thus environmental hypotheses did not inhibit the development of quantification in the same way as phylogenetic speculations in biology. In fact the two schools were not particularly distinct during the simple descriptive statistic stage in regional taxonomy with Whittlesey, for instance, contributing to both approaches.<sup>65</sup> However, with the use of more sophisticated techniques by the taxonomists the two methods

have grown apart so that dialogue between the schools is strained and marked with misunderstandings of basic methodological positions on both sides. Examples of this sterile dialogue are Minshull's criticisms of Haggett's regionalizing in a chapter on the mapping approach to regional delimitation<sup>66</sup> and Russell's subsequent critical review<sup>67</sup> of Minshull's very traditional Oxfordian statement of regional geography.

Having made this important distinction between 'regional geography' and the taxonomic approach the remainder of this essay is concerned with the latter. The recent quantitative revolution in this field has accompanied a methodological shift in geographic science as a whole rather than a methodological change in taxonomic thought in particular. This fact is represented in the quantitative trends described above by the lack of any time-lag in regional taxonomy as related to other geographic studies. However, an interesting paradox can be identified here. Whereas modern geography has become more theoretically orientated, regional taxonomy has largely followed an empirical line similar to, although not as explicitly extreme as, that of Sokal and Sneath. For instance, Harvey considers regionalization simply as an 'observational model'<sup>68</sup> in his recent book. This paradox merits further investigation since it leads directly on to an alternative interpretation of classification than that offered by modern quantitative taxonomists.

#### The paradox of quantitative taxonomy

It is clear that the rise of a quantitative taxonomy had been the result of a conscious effort by practitioners in several fields to make classification more 'scientific'. This is particularly true in the case of Sokal and

Sneath who base their claim on "two outstanding aims" namely 'objectivity' and 'repeatability'.<sup>69</sup> However one critic has claimed that:

"A taximetric approach to classification may be interesting from the mathematical point of view, but in the majority of cases it does not provide more useful taxonomic information than any other system of classification based on correlation of phenetic features".<sup>70</sup>

Certainly if classification is to be 'more scientific' it should have some specific role in the development of the various empirical sciences beyond simple empirical data sorting.

#### 1. Classes and concepts.

The case for a role for classification in scientific explanation has been most ably argued by Hempel. He contends that classification can be viewed as one approach to concept formation in empirical science.<sup>71</sup> He develops his case most fully in his paper aptly titled "Fundamentals of Taxonomy"<sup>72</sup> which has the distinction of being largely ignored by taxonomists including all of those referred to in this essay. The arguments of this section are primarily based on this hitherto neglected paper.

Hempel regards every class as the extension of a more theoretical concept. Thus although he agrees that objectivity and 'inter-subjectivity' are important he goes on to comment that:

"clear and objective criteria are not enough. To be scientifically useful a concept must lend itself to the formulation of general laws or theoretical principles which reflect uniformities in the subject matter under study, and which thus provide a basis for explanation, prediction and generally scientific understanding."<sup>73</sup>

This is a far cry from the empiricism of much recent taxonomy. Thus Sokal

and Sneath's idea of efficiency based on time and cost are replaced by the traditional criteria for judging the usefulness of concepts - what Hempel terms 'systematic import'.

The examples Hempel uses to support his arguments are particularly interesting and are taken from classification of diseases, animals and chemical elements. In the former case, he notes how classification has developed "from a largely symptomatalogical to a more and more etiological point of view".<sup>74</sup> For instance, Kaplan, who follows Hempel's paper in his discussion of classification, notes how the class 'epilepsy', a purely empirical descriptive class, has been an almost useless concept because it labelled several distinct diseases that happened to produce similar symptoms.<sup>75</sup> However, in the context of this essay, Hempel's most interesting reference is to Simpson's work. He uses an example from animal taxonomy to show how phylogenetic classification is preferable to simple phenetic classification. Thus Hempel agrees with both the objectivity aims of numerical taxonomy while justifying the more traditional evolutionary taxonomy. This seemingly conflicting situation is the same paradox we have identified as existing in recent geographical research and may be generally termed the paradox of quantitative taxonomy. On the one hand quantitative taxonomy attempts to be objective and thus scientific while its procedures tend to produce classes of less theoretical importance than the traditional taxonomist carrying out his subjective art based on some a priori theory.

The cause of this paradox can be traced back to the methodological background discussed above. In regional taxonomy, for instance, it can be

argued that the genetic classification of soils and landscape features have more 'systematic import' than their generic counterparts since at least they attempt to relate to scientific explanation even if the theories on which they are based are weak. The situation in human geography is somewhat simpler. Since the environmental determinism of early regionalization had been discredited long before recent quantitative trends, the absence of any other theoretical framework to take its place has led to a single empirical approach to classification. However, in any case, as was noted above, a definite paradox exists here, as in biological taxonomy, as between developing scientific explanation and the practise of modern taxonomy.

Following on from Hempel's equating classes with a certain type of concept, the paradox described above can be interpreted as a special operational case of what Kaplan has termed the paradox of conceptualization.<sup>76</sup> Quite simply "proper concepts are needed to formulate good theory, but we need a good theory to arrive at proper concepts." Biological taxonomy clearly brings out this point where existing theory is not strong enough to produce satisfactory inter-subjective classes. This is because evolutionary theory has not developed sufficiently to distinguish homology, convergence and parallelism among species without empirical evidence which, as Sokal and Sneath note, is most usually lacking. Thus the researcher is either forced into the pure empiricism of the numerical taxonomy school or subjective 'theoretical' classification of the more traditional taxonomy.

## 2. The method of successive approximation.

The way out of this paradox seems to be a mixture of these two approaches.

Kaplan writes:

"Like all existential dilemmas in science, of which this is an instance, the paradox is resolved by a process of approximation: the better our concepts, the better the theory we can formulate with them, and in turn, the better the concept available for the next improved theory."<sup>77</sup>

Lenzen<sup>78</sup> terms this process 'successive definition' and in the taxonomic context this is equivalent to Hennig's "principle of reciprocal illumination"<sup>79</sup> whereby phylogenetic and phenetic considerations each have a place in the classification process as a system of classes are progressively refined.<sup>80</sup> Although Sokal thinks this approach is 'questionable' the extreme empiricism of numerical taxonomy does fit into this scheme as a necessary approach at the lower end of the ladder leading towards more sophisticated theory.

This interpretation now raises an interesting point concerning classification in general. The taxonomy usually taken as the basic paradigm is itself in a state of some confusion<sup>81</sup> since it has not progressed far in the 'reciprocal illumination' of classification and theory process. This being so it is natural to question whether biological taxonomy presents a suitable paradigm for other classifications to compare with or even emulate. Ideally a classification is needed that has contributed to and is part of some theoretical structure and is thus some way along the path of successive approximation. The taxonomy that meets this requirement is the chemist's periodic system of elements.

#### An alternative paradigm: the periodic system of elements

Undoubtedly the most successful classification in science has been the periodic system of elements. Despite this fact, one hundred years after

its discovery it is still largely ignored in methodological discussion of classification with the notable exception of some references to it by Hempel, noted above. For this reason this section begins with a brief description of the system before its development is discussed and it is related to other taxonomy.

The periodic system consists of an arrangement of chemical elements in an n space in terms of their atomic number and mutual affinities. The space is typically two dimensional and is thus often referred to as the 'periodic table of elements' although several three dimensional models have been proposed. In a two dimensional table, elements are arranged according to the 'periodic law' which states that "the properties of chemical elements are a periodic function of the atomic number."<sup>82</sup> This means that the properties are repeated at fairly regular intervals (or periods) in a listing of the elements ranked according to their atomic numbers. Thus elements are laid out in the form of horizontal and vertical relationships and the classification can be defined as "a system in which all the elements are arranged according to increasing atomic weight and elements with analogous properties occur in columns or groups."<sup>83</sup>

#### 1. The history of the system.

The historical development of the periodic system of elements has been traced in detail by van Spronsen<sup>84</sup> and this section is largely drawn from this source.

The discovery of the periodic system followed on from two parallel developments in chemistry.<sup>85</sup> First there was the tradition of classification



of chemical substances from Aristotle through to Linneaus and into the nineteenth century. The second trend was one of deriving numerical relationships between elements which was largely a nineteenth century development. In particular the periodic system had to wait, first of all, the definition and discovery of a large number of chemical elements to act as the basis of the classification replacing the compounds used by the taxonomists such as Linneaus. Secondly, once a sufficient number of the basic units were known, a knowledge of the quantitative relationships between them was necessary. It is these experimental laws that form the rationale of the system. This led to the development of several smaller systems of elements in the 1860's although the discovery of the periodic system is usually credited to Mendeleev in a famous paper presented to the Russian Chemical Society in March 1869.<sup>86</sup> This was, in Guerlac's terms, "the final triumph of quantification in the older chemistry."<sup>87</sup>

In contrast to the sophisticated multi-variate techniques employed in modern taxonomy the procedure used in classifying the elements has been termed 'a groping process.'<sup>88</sup> However, researchers here had the advantage of a set of known 'experimental truths' so that the periodic system was "the product of chemistry in all its aspects, and all chemical laws influenced its discovery."<sup>89</sup> Thus the classification was not based on mathematical axioms or theses or on theoretical hypotheses but rather on empirical laws. Mendeleev is explicit on this point and states that he introduced no hypotheses in his system of elements.<sup>90</sup> However, the system was not a 'once and for all' classification of the elements. In the priority conflict between

Mendeleev and the German chemist Meyer, for instance, Mendeleev contends that Meyer did not appreciate "the deeper meaning" of the system.<sup>91</sup> By this he meant that Meyer had not originally appreciated that the system of elements could be used for more than mere classification. In fact one of the most celebrated aspects of Mendeleev's original paper was that it predicted the existence of new elements not known in 1869 and was even able to predict their properties on the basis of his system:

"From the very beginning, Mendeleev took the existence of still undiscovered elements into account and arranged his classification accordingly."<sup>92</sup>

Thus in his 1871 system he left a large number of vacant spaces and predicted properties of elements that were subsequently discovered. However, Mendeleev's system was never considered a rigid framework so that when new unpredicted elements were discovered, such as the noble gases in the 1890's, these were finally able to find a place in a revised system. Similarly the replacement of the Dalton atom by Bohr's atomic theory meant replacing atomic weight by the ordinal atomic number but this led to no fundamental rearrangement of the system. Thus the periodic classification has survived into the era of modern atomic theory despite its nineteenth century antecedents. In fact, its most notable and, perhaps most important, characteristic is its flexibility within a single basic structure. For instance, Mazurs<sup>93</sup> is able to identify a total of 110 different forms of table which fall into thirty fundamental types. However, all these systems are based on the periodic relationship between atomic number and the properties of elements. As van Spronsen states:

"In the end it became apparent that this method of classification was not artificial; every element received a place in the natural order. The many empirically established properties of elements finally became part of a general law, the periodic law, as a 'natural classification of elements'." <sup>94</sup>

## 2. The periodic system and other taxonomy.

Obviously our basic interest here is to compare the development of the 'natural classification of elements' to other classifications. In this section the discussion will be of a general nature with examples drawn largely from biological taxonomy since specific consideration of regional taxonomy is left to the next section. The general comparison is introduced by dealing, in some detail, with the purposes these classifications have assumed.

Mendeleev emphasized the many applications of his system among which the following stand out: <sup>95</sup>

- (1) to classify the elements;
- (2) to determine atomic weights of elements not sufficiently analyzed;
- (3) to examine properties of unknown compounds;
- (4) to correct erroneous atomic weights; and
- (5) to collect information on properties of compounds.

Mendeleev himself made use of all these properties. Thus Ihde <sup>96</sup> has noted that the system has been "much more than mere classification" it has been - "conceptual tool" in the scientific structure of modern chemistry.

This explicit multi-purpose role of the periodic system contrasts markedly with the recent approach of numerical taxonomists. Thus Sokal and Sneath criticize the traditional taxonomy because it attempts to achieve

too many purposes and as a result does none of them well.<sup>97</sup> Thus numerical taxonomy attempts only the first application Mendeleev mentions - simple descriptive classification.

These two contrasting opinions regarding the role of a classification system can be traced to the very apparent differences in the sophistication of the two respective sciences. Thus whereas "the periodic system rested on a solid empirical basis" there are no equivalent empirical laws in other taxonomies to produce such a useful classification. This basic difference between the proposed paradigm and other classifications does mean that the paradigm can act only as a possible model towards which to aim and not as a system that can be directly, or even indirectly, emulated in the less developed sciences. Thus the divergence between Sokal and Sneath's single descriptive purpose and the more ambitious role of Mendeleev's system are quite compatible given the respective levels of knowledge in the two fields.

However, this section will be concluded on a more optimistic note by pointing out some of the similarities between recent taxonomic work and the periodic system. First of all, it should be noted that the development of the system was not as smooth and logical as the simplified account above might suggest. Obviously this section has emphasized the successful, constructive contributions in the history of the elements. However, van Spronsen, in his introduction to the system, assures his readers that they will "be astonished at the nonsensical hypotheses, incomprehensible errors, and faulty interpretations made, before and after its discovery, by many investigators."<sup>98</sup> Of much more relevance, however, is the fact that the development

was largely empirical in nature. Thus Sanderson goes as far as to state that:

"Like many another great scientific discovery, the Periodic law was discovered entirely empirically, and long before its fundamental basis was understood."<sup>99</sup>

The latter part of Sanderson's statement refers to the fact that the periodic system was able to develop when the current atomic theory was that of Dalton. This theory was inadequate for giving a theoretical basis to the system so that for nearly half a century after its discovery the periodic system was not fully understood. This had to await the replacement of Dalton's theory by the Bohr model in 1913.

All this latter discussion seems to justify an early empirical emphasis for developing scientifically useful classifications within an overall strategy of successive approximation. The numerical taxonomy school of biology certainly fits this role even if the practitioners do not favour Hennig's reciprocal illumination.

#### Regionalization and theoretical geography

This final section attempts to assess the role of regionalization in modern geography by relating some of the themes and ideas from previous sections to regional classification.

The first question must be whether the region has any worthwhile contribution to make to modern geography. Turnock<sup>100</sup> has recently answered this question with a very emphatic 'yes' although this answer seems to be largely based on practical grounds with no discussion in terms of modern trends towards geographic theory. However, this reluctance to relate

regional classification to geographic theory can be found in the work of more theoretically orientated geographers. Thus both Schaefer<sup>101</sup> and Harvey<sup>102</sup> are concerned solely with an empirical role for regions. In the former case, the region is to act as a 'laboratory' in which the geographer empirically tests his theories and in the second case Harvey discusses classification as an 'observation model' for conveniently arranging large data sets. In fact Harvey explicitly follows Sokal and Sneath's criticism that traditional taxonomy tries to achieve too many purposes,<sup>103</sup> an argument leading directly onto the extreme empiricism of numerical taxonomy since it is the theoretical purpose of classification that is dropped. Thus, taking this view, regionalization is just one of several multi-variate techniques employed in modern geography. If this is the case then the region idea has lost much of the importance it has traditionally held in geography as, in fact, Gould's review of recent methodological developments would seem to suggest.<sup>104</sup>

However, not all recent discussions of regionalization have defined it as a purely empirical exercise. Grigg, in particular, stresses that the most important function of a classification is for it to be used for making generalizations.<sup>105</sup> This point is largely ignored by Harvey.

Of course Harvey is by no means alone in omitting to incorporate classification in methodological discussions of theory construction. The problem can be traced back to the fact that the conventional paradigm for discussing theory is based on practice in physical science whereas classification is usually related to biological research activity. Thus classification does not fit neatly into standard discussions of theory construction. For

instance, Jevons,<sup>106</sup> in his nineteenth century treatise of scientific methodology, includes a section on classification as the last substantive chapter of the book so that it is not integrated with any of his previous discussion. However, the fact that classification has less importance in developing physical theory by no means precludes the possibility that it may be relevant to theoretical geographers given the present level of geographic theory.

1. A chemical analogy?

The example of the periodic system of elements shows that classification can have a place in the more developed sciences. Thus, the specific question arises as to whether this most sophisticated classification can act as a model for researchers for regional taxonomy. Although this possibility has been doubted for other taxonomy above, there can be found in geography, an early attempt to draw an analogy between chemical classification and the region concept. Weaver,<sup>107</sup> in 1954, equated crop types with chemical elements thus suggesting that the agricultural region, as a combination of crop types, is directly analogous to the chemical compound. However, no mention is made of the periodic system of elements where emphasis lies with the elements, and compounds are used merely to give information (e.g. valency) about elements. Taking Weaver's analogy to its logical conclusion would therefore seem to suggest that it is not the regions that are important but the abstract elements of which they are made up.

However, as in biological taxonomy, regional taxonomy has no body of empirical laws comparable to those existing in nineteenth century chemistry.

In fact, only one such law can possibly be claimed - Tobler's so called 'first law of geography' that near objects tend to be more related to one another than distant objects<sup>108</sup> - so that the development of any scheme even remotely approaching the chemical model seems to be a possibility that can be largely ignored here. Thus instead of developing any direct analogy with chemical classification the emphasis must unfortunately be concerned with Kaplan's paradox of conceptualization and the early stages of successive approximation or more hopefully 'reciprocal illumination'.

2. 'A priori' classification.

Thus far discussion has been related to the recent multi-variate classificatory techniques associated with the new quantitative taxonomy. With these techniques the classes are not known beforehand and the technique 'produces' a system of 'objective' classes from the data. This is termed 'typology' by Gilmour and Walters.<sup>109</sup> Since some 'reciprocal' approach is assumed to be relevant to this discussion it is necessary to briefly consider the alternative 'subjective' approach to classification. This is the 'definitional' approach of Gilmour and Walters<sup>110</sup> whereby a system of classes is specified beforehand and then objects are allocated to these known classes. Obviously such an approach enables classes to be directly related to concepts as envisaged by Hempel. The most explicit use of this method can be found in Lazarsfeld and Barton's<sup>111</sup> discussion of classifying responses to questionnaires. In geography, climatic classifications have usually been of this form.<sup>112</sup> However, when this approach is linked with weak deductive theory as in biological taxonomy and Davisian geomorphology



then the result has been the phylogenetic, or simply genetic, classifications so strongly criticized by recent researchers.

In this situation, where there is very little empirical evidence to support any hypotheses, a possible starting point would seem to be what may be termed 'combinatory' a priori classification. In this approach no theory is involved and classes are defined by using all possible combinations of variables. The fact that many classes will be defined in which no object fits can be taken as being the first evidence for rudimentary theoretical explanation.

This approach is explicitly used in Golledge and Amadeo's short discussion of set theory and regionalization<sup>113</sup> and is implicit in Weaver's method of defining crop combination regions.<sup>114</sup> Thus Weaver's combination type regions are not initially restricted in terms of crop combinations so that all possible combinations of crops make up the a priori set of classes into which counties are allocated.

Since King<sup>115</sup> has recently used Weaver's original land use data to illustrate one of the more recent multi-variate typological approaches,<sup>116</sup> Weaver's approach will be used to illustrate the basic advantage of the definitional method in terms of conceptual clarity. Although King points out that his typological solution for four regions is very similar to Weaver's four crop combination regions, the fact remains that definite statements can be made about Weaver's regions that are not necessarily true about King's regions. Thus if a county falls into Weaver's three crop Wheat-Barley-Oats region it is known that this county conforms to a particular ideal model in

terms of specified criteria.<sup>117</sup> On the other hand if this county falls into King's roughly equivalent region, no specific statements can be made about the county except that it occurs near various other counties within a four dimensional factor space. This same argument can be used with respect to comparing Koppen's climatic classification of North America with Casetti's iterative improvement<sup>118</sup> and of course traditional monothetic biological classification and numerical taxonomy.<sup>119</sup> Thus the a priori approach, despite a great deal of criticism in recent years, automatically leads to less 'fuzzy' classes and therefore, ultimately, clearer concepts. This basic advantage must be weighed against the disadvantages enumerated in detail above. However, the argument here is not for one method or the other but rather for a combination of the two.

### 3. Successive approximation: central place theory and urban ecology.

In the past regional taxonomy has not been closely related to geographic theories or models. This is inspite of the fact that central place theory, for instance, predicts systems of regions as its resulting spatial structure. Here we consider two areas of geographic model building, one where regional taxonomy has made little or no contribution and another that seems to fit Kaplan's successive approximation pattern quite well.

In central place studies functional regions have been produced by a definitional approach based on one of the theory's postulates (consumer distance minimization) in the method of Thiessen polygons<sup>120</sup> and typological approaches have been applied to flow data to produce functional regions.<sup>121</sup> However despite the existence of both approaches in central place studies

there seems to be no sign of any 'reciprocal illumination' and regional taxonomy studies seem to have contributed little or nothing to the development of central place theory. This is a disappointing conclusion from the taxonomic viewpoint so that it is encouraging that we can balance it with something of a contrasting situation.

This section is concluded with a case study of an area of regional taxonomy which has progressed in a manner not unlike Kaplan's successive approximation. The example that seems to fit this pattern is the development of regional urban ecology from Park and Burgess through to the recent factorial ecology studies. On the barest outline of this research is presented here since these developments are discussed in more detail elsewhere in this report.

The first step in this sequence is the postulates of Burgess, Hoyt and Ullman and Harris<sup>122</sup> that were largely based on inspection of empirical map evidence. These models of spatial structure represent the first empirical stage.

The second stage involves the use of urban social theory to develop Shvky and Bell's social area analysis.<sup>123</sup> This is the first definitional classification stage whereby three basic 'a priori' dimensions are defined and census tracts allocated accordingly.

The second empirical stage is represented by the large number of factor analyses recently carried out on urban ecological data.<sup>124</sup> These results have enabled Berry<sup>125</sup> to relate both Burgess and Hoyt's models to the Shvky-Bell classification. Thus factorial ecology can rightly claim to

be a culmination of both these previous research efforts. The stage would now seem set for a second definitional stage based on more sophisticated theory incorporating the findings of the recent factor analyses.<sup>126</sup> However, leaving aside predictions of future research advances, urban ecology does present a neat example of successive approximation and even 'reciprocal illumination' in the development of an explanation of urban spatial structure.

#### Concluding Remarks

The debate over the validity of the taxometrics approach has tended to center on a rather sterile dialogue concerning the 'objectivity' of numerical procedures<sup>127</sup> to which several geographers have contributed.<sup>128</sup> This essay has attempted to go beyond this concern over procedure and concentrate rather on purpose. Thus it seems logical to conclude this chapter by introducing a typology of taxonomic approaches based on purpose rather than procedure. Such a typology would seem particularly useful since statements on principles of taxonomy invariably stress 'purpose' as of paramount importance.<sup>129</sup>

Although we have questioned many of the assumptions of recent quantitative taxonomy, the arguments should not be regarded as anti-quantitative as such. There is no suggestion here that a purely empirical approach is in any sense incorrect since there is obviously a need for such taxonomy to produce data storage systems as the amount of information greatly increases. Solutions to this type of problem can be viewed as one of three basic approaches to taxonomy. Edwards and Cavalli-Sforza<sup>130</sup> term these 'pure taxonomy.'

'applied taxonomy' and "phylogenetic speculations" (evolutionary taxonomy) which we can generalize to 'theoretical taxonomy.' Pure taxonomy is concerned solely with the processing of information and would seem to encompass the numerical taxonomy school of Sokal and Sneath. Applied taxonomy seeks to classify for a specific purpose at hand and is often referred to as 'special purpose classification' while theoretical taxonomy is more general and has been what we have concerned ourselves with here - the use of a classification as part of the explanatory apparatus of a science.

This typology of approaches has much to commend it beyond its transcending the quantitative-non-quantitative debate. Usually special purpose classification is considered as simply the opposite of general purpose classification often referred to as 'natural.' This dichotomy has been plagued by two interpretations of 'natural' classification - Sokal and Sneath's multi-variate definition<sup>131</sup> and Simpson's 'non-arbitrary' definition based on clear 'gaps in resemblance' indicating evolutionary separation.<sup>132</sup> By converting a previous dichotomy into three types of approach Edwards and Cavalli-Sforza would seem to have avoided the 'natural' classification debate by including the multi-variate approach as pure taxonomy and the non-arbitrary approach as theoretical taxonomy.

This three way typology would also seem to be a useful complement to the usual discussion of regions as either formal (homogeneous), functional, and programming or applied regions.<sup>133</sup> The latter is identical to applied taxonomy as defined by Edwards and Cavalli-Sforza and would seem to fit

into the latter's scheme much better since it is defined in terms of the purpose of the regionalizing and not the method of defining the regions. Programming regions can be either 'formal' or 'functional' in character for instance so that the traditionally triology does not conform to basic taxonomic principles.<sup>134</sup> Combining the Edwards and Cavalli-Sforza typology with the formal-functional dichotomy produces 'pure regionalizing' (locational classifications for information storage), 'special purpose regionalizing' (definition of experimental and programming regions), and 'theoretical regionalizing' (identification of regional classes as empirical extensions of theoretical concepts) each of which may be formal or functional in structure. The final comments in this essay will concern the current trends and future status of the two contrasting 'pure' and 'theoretical' approaches.

The current trend in publications in quantitative taxonomy is for more and more contributions to appear in computer science journals.<sup>135</sup> Such a situation can be viewed as a logical extension of the extreme empiricism of Sokal and Sneath and has led to the suggestion that pure taxonomy is not really 'taxonomy' at all<sup>136</sup> but rather part of the exploding realm of computer science and its associated disciplines. A recent symposium on numerical taxonomy under the auspices of a computer science institution rather than a biological school would seem to add support to this view.<sup>137</sup> Thus we can expect a future pure taxonomy consisting of systems of algorithms for efficiently storing information so that it is readily available for subsequent processing.<sup>138</sup> Such a scheme will be independent of

theories relating to the subject matter being classified and stored.

However, not all the evidence points to the development of a pure taxonomy out of the quantitative revolution in taxonomy. Several examples can be quoted where computer scientists and researchers in other fields have combined to co-author taxonomic papers.<sup>139</sup> Geography has recently furnished two such examples<sup>140</sup> and this interdisciplinary work suggests quantification in taxonomy may yet have something to offer to successive approximation procedure in theoretical taxonomy. The urban ecology example is particularly instructive in this context.

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129. This is Grigg's first 'principle of classification' ; Grigg, "The logic... op. cit., 1, p. 481.
130. Edwards and Cavalli -Sforza, op. cit., 46, pp. 69-71.
131. Sokal and Sneath (op. cit., 11) talk of "the reason for the great usefulness of natural classifications is that when the members of a group share many correlated attributes, the 'implied information' or 'content of information' is high", p. 19.

132. Simpson, op. cit., 49, pp. 114-9 and P. C. Simpson, "The species concept," Evolution, 5 (1951), pp. 285-98. See also Spence and Taylor, op. cit., 3, pp. 27-8.
133. Given for instance in Berry, "Essays on ..." op. cit., 3, p. 189; J. R. Boudeville, Problems of Regional Economic Planning, Edinburgh: Edinburgh University Press, chapter 1; and W. Bunge, "Theoretical Geography," Lund Studies in Geography, Series C, 1, (1966, second revised edition), pp. 14-26. The latter author identifies four types of region adding 'experimental region' to the usual three. However, experimental regions can be accommodated under the special purpose category along with applied regions.
134. The principle involved here is that classes should be defined in terms of a single approach at any one level of a hierarchy - Grigg "The logic...", op. cit., 1, p. 481.
135. The post-1965 references in Spence and Taylor, op. cit., 3, are evidence of this trend.
136. By W. T. Williams in his "discussion" of numerical taxonomy in Heywood and McNeill (eds.), op. cit., 34, p. 79.
137. A. J. Cole (ed.), Numerical Taxonomy, London and New York: Academic Press (1969). This is the Proceedings of the Colloquium in Numerical Taxonomy held in the University of St. Andrews, September, 1968.
138. For instance, D. Wishart, "Fortran II programs for 8 methods of cluster analysis (CLUSTAN I)," State Geological Survey, University of Kansas, Computer Contribution, 38.
139. For instance, R. M. M. Crawford and D. Wishart, "A rapid multi-variate method for the detection and classification of groups of ecologically related species," Journal of Ecology, 55 (1967), pp. 505-24.
140. D. C. D. Pocock and D. Wishart, "Methods of deriving multi-factor uniform regions," Transactions, Institute of British Geographers, 47 (1969), pp. 73-98; A. J. Wilmott and P. N. Grimshaw, "Cluster Analysis in Social Geography," in Cole (ed.), op. cit., 137, pp. 272-81 and P. N. Grimshaw, M. J. Shephard and A. J. Wilmott, "An application of cluster analysis by computer to the study of urban morphology," Transactions, Institute of British Geographers, 51 (1970), pp. 143-62.

## CHAPTER VI

### LOCATION THEORY -- DEVELOPMENTS ON BEHAVIORAL FOUNDATIONS

#### Quantitative Character of Classical Location Theory

At the time of the "quantitative revolution" in the 1950's there already existed a rich location theory literature the authors of which used quantitative approaches both in the development and in the testing of their respective theories. The classical literature was primarily deductive from either postulates of behavior long used in economic theory or from new postulates of spatial behavior consistent with the assumption of cost minimization on which the former group of postulates were based. A large literature in agricultural and industrial location theory had been developed in the nineteenth century,<sup>1</sup> while the literature relating to the location pattern of service activities (central place theory) was developed by Christaller and Lösch in the nineteen-thirties.<sup>2</sup> In all three cases the broad methodological approach was similar. This can be categorized as:

- 1) make assumptions about the character of the environment in which the locational pattern of activities is to be developed - the initial conditions of the theory;
- 2) declare some behavioral postulates which the various "actors" in the system are to follow;
- 3) define constraining conditions -- often inductively derived, to control the way in which the spatial system will be developed;
- 4) apply 2) and 3) to 1) and deduce expected characteristics of the spatial pattern in terms of both location and mix or type of activity -- the theorems of the theory.

An Illustration: Deriving Spatial Patterns from Behavioral Propositions

Agricultural Activity Patterns

From quite simple decision-making rules, spatial patterns can be derived that quickly become complex when the choice rules are applied to environments in which relevant variables vary markedly through geographic space. For example, the choice rule that a farmer will engage in those activities that will give him the highest return has been interpreted as the statement that the farmer will choose the activity that maximizes the value  $R$  in the expression<sup>3</sup>

$$R = E(p-a) - Efk \text{ -----(1)}$$

where:  $E$  is yield per unit area  
 $p$  is price c. unit of commodity  
 $a$  is cost of production of unit of commodity  
 $f$  is transfer cost per unit distance per unit of commodity  
 $k$  is distance to market

Von Thünen<sup>4</sup> showed how in any area where productivity and costs (other than marketing costs) do not vary through space, activities will assume a concentric zone arrangement. However, when yields and costs of production vary through space, the spatial pattern that results can take on an infinity of forms. Insofar as the patterns of yields and costs repeated itself from one area of space to another, so patterns of activities could be found. Geographers of the "spatial analysis" school were thus able to identify some recurring patterns. Chisholm,<sup>5</sup> for example, reviewed many studies that had reached the conclusion that intensity of land use declined systematically with distance from local villages and, at a larger scale, with distance from major market centers. More recently, Blaikie<sup>6</sup> has investigated the same relationships

in India.

Before the widespread availability of computers, the application of equation (1) to a geographic area where the variables in question varied in unique ways, was, practically speaking, impossible. This undoubtedly fostered the attitude of many that such location theories were interesting pedagogical devices without the potential for application to the real world. However, while this attitude is still prevalent, three developments have made it obsolete. First, developments in mathematical generalization of statistical surfaces allowed unique configurations of spatial patterns of variables to be expressed as a function of the locational coordinates of the area.<sup>7</sup> (See Chapter IV for a discussion of these developments). Second, the development of regional data banks has raised the likelihood that large areas of the world will soon have related to them a wide range of information on current and past levels of economic activities in quick, machine-accessible form.<sup>8</sup> Third, it is now possible to computerize the application of geographical models, as illustrated in equation (1), to earth environments.

#### Summary of Classical Approach

In this illustration of the location of agricultural activities, the classical treatment of equation (1) will be shown first and this will be followed by an illustration of how the theory may be transformed in both its generality and in its potential for application to real situations, after the three developments described above have been incorporated.

In the expression:  $R = E(p-a) - Efk$  described above, where all variables are constant except  $k$  (distance),  $R$  will always decline as a linear

function of distance from the market (Fig. 1). If we hypothesize that there is no competition with a particular activity and that that activity will be found wherever economic rent is greater than zero; then  $R=0$  where  $E(p-a) = Efk$ ; i.e., where  $k = \frac{E(p-a)}{Ef}$ , i.e., where  $k = \frac{p-a}{f}$ . Thus, at all distances less than  $k$  (where  $k = \frac{p-a}{f}$ ) economic rent will be greater than zero. Since we have hypothesized yields, prices and costs to be constant through space, total area under productivity (A) can be calculated as  $A = \pi \left(\frac{p-a}{f}\right)^2$  and total supply to the market (S) as  $S = E \pi \left(\frac{p-a}{f}\right)^2$ . Where several activities exist, computation of areas under production is not so simple; however, Fig. 2 shows a graphical solution for this case.

An interesting question is that of how spatial patterns of production change in response to changes in product price. Dunn<sup>9</sup> has shown how the analysis just described leads to results that are consistent with classical (aspatial) economic theory for the equilibrium conditions of supply and demand. Clearly, if price increases, all other variables remaining constant then both the area in production (A) and the total amount supplied to the market (S) will increase. This result leads to positive sloping supply curves and if we postulate that local demand will be negatively sloping then we see that there is a point of equilibrium for which there is a corresponding spatial pattern of production. In Figure 3 both the activity supply curve and the local demand curve are shown. The equilibrium point represented in Figure 3 is known as the space-price equilibrium. It is an equilibrium point because any departure from the point will be accompanied by forces which lead to a return to the point. If, for example, in Figure 3,  $P_1$  is the prevailing price, then  $S_1$

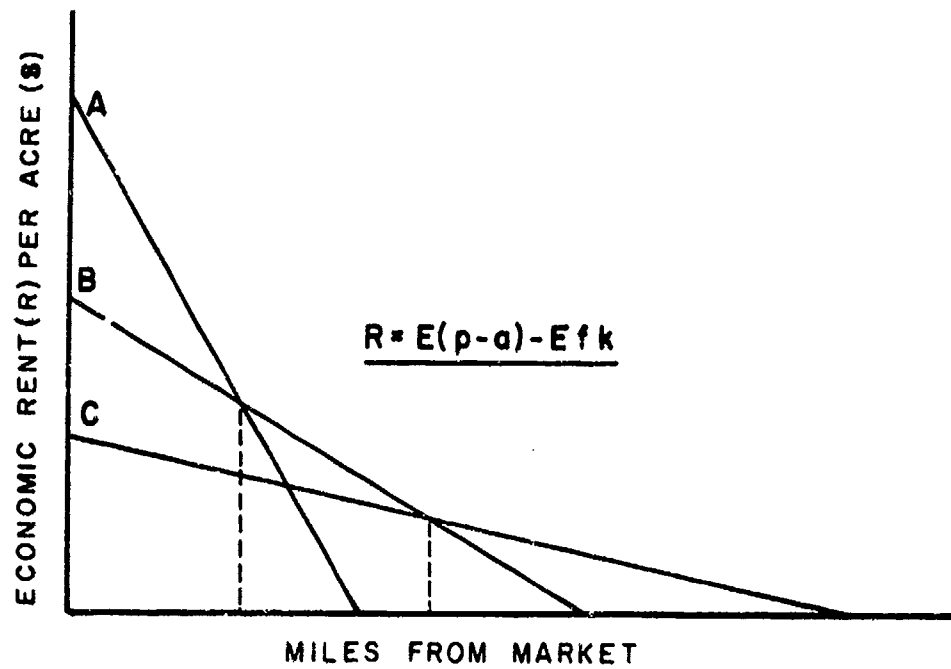


Figure 1: Three Hypothetical Rent Gradients

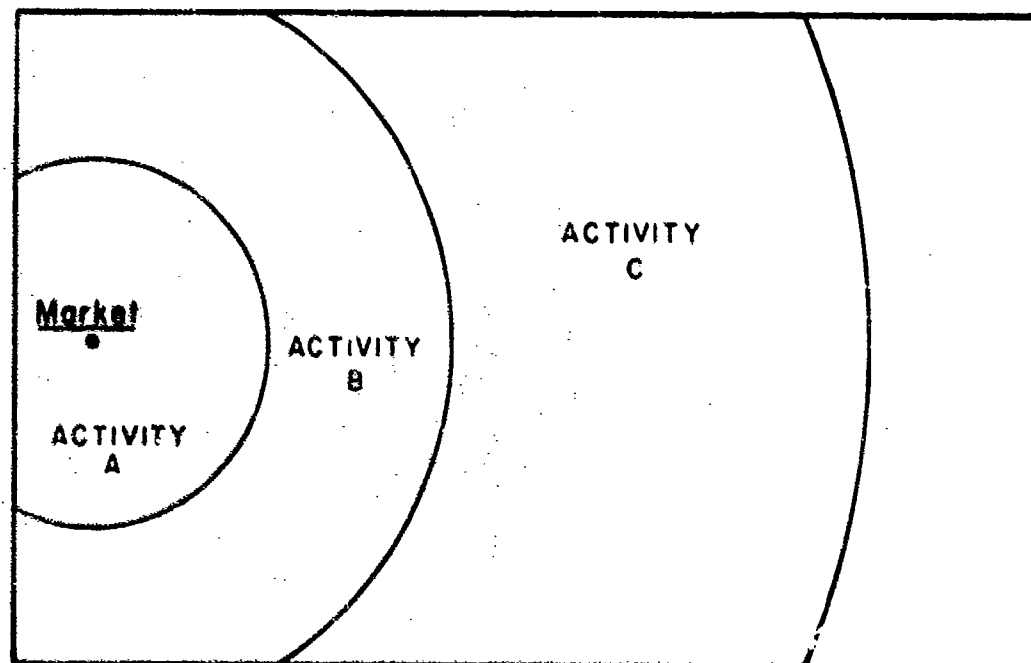


Figure 2: Associated Spatial Pattern

would be the amount supplied whilst a smaller amount ( $D_1$ ) would be demanded by consumers. This position is obviously not in equilibrium for it would lead to a surplus of the commodity in question. The most common method of disposing of a surplus is to offer it at a lower price and, as we have seen, a lower price would lead to a smaller supply by producers; marginal producers either going out of business or switching to some other activity.

#### Changes in Space-Price Equilibrium

The process which leads decision-makers to switch from one activity to another is of central concern to us. Suppose two activities are in space-price equilibrium as defined in Figure 3 and are distributed according to rent gradients shown in Figure 2. Consider the circumstances which might disturb the space-price equilibrium of one activity and let us see how a new space-price equilibrium might be expected to evolve and also how this new equilibrium point would in turn disturb the space-price equilibrium point of the second activity. The world we live in is, of course, made up of more than two activities and, unfortunately for our attempts at comprehension of the underlying order in it, is a world in which there are very many disturbing forces. However, using von Thünen's method of idealization it is possible, by conceptualizing the essence of the problem and reducing the number of variables at work, to discover the way in which these forces are mutually related and resolved.

The circumstances we might postulate as changing the space-price equilibrium of the activity might be a change in demand at a given price.

There are many different circumstances which might bring this about; suffice



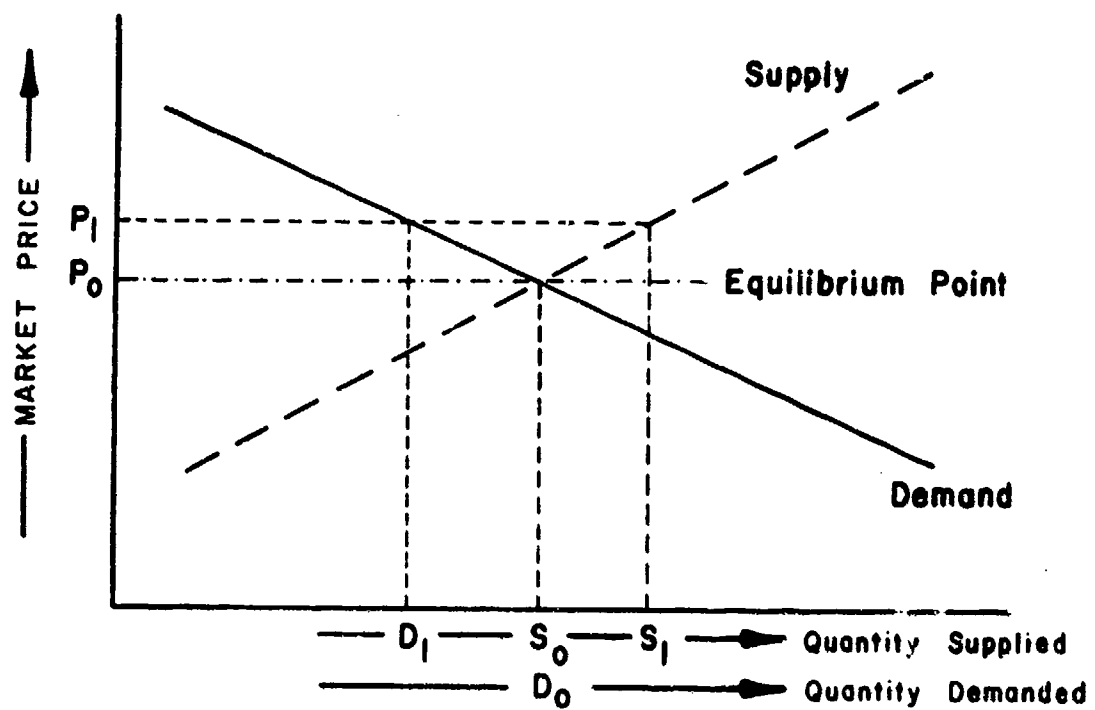


Figure 3: Space-Price Equilibrium

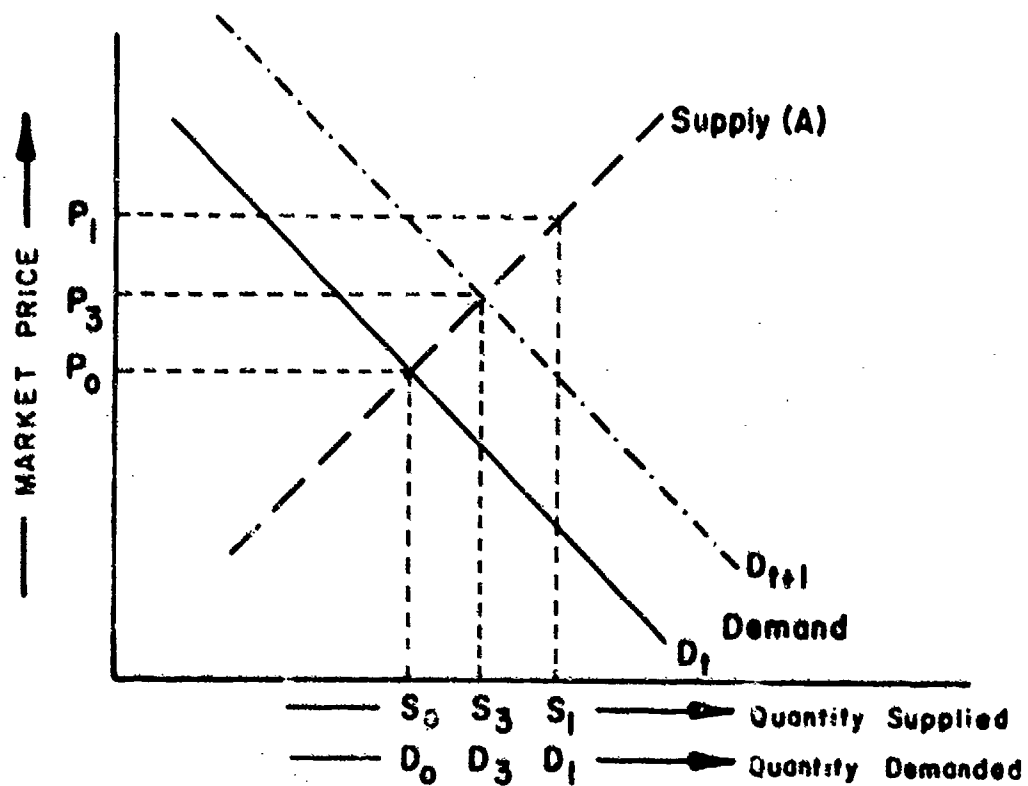


Figure 4: Changes in space-price equilibrium of activity A following an increase in demand for A at a given price

at this point to say that an increase in the population of the area or a change in tastes and preferences favoring the product of the activity in question would be sufficient for this postulated change in demand. Such a change is not to be thought of as a movement along the demand curve but rather as a movement from one demand curve to another. In Figure 4 this is shown as  $D_t$  - the original demand curve; and,  $D_{t+1}$  - the revised demand curve following from the changed circumstances which were postulated. In that figure the new demand,  $D_1$ , exceeds the supply at price  $P_0$  which is still  $S_0$ . Only at the new price of  $P_1$ , would the supply ( $S_1$ ) equate with demand at price  $P_0$ . However, at the new price of  $P_1$ , the amount is  $D_0$ , which is less than  $D_1$ . Thus the forces described in Figure 3 are the ones which would lead to the new space-price equilibrium point with price  $P_3$ , supply  $S_3$ , and demand  $D_3$ .

The accompanying spatial changes in this hypothetical example still have to be demonstrated. The question to be answered is what will be the effect of a price change on the economic rent gradients of Figure 2? This question can be answered by substituting the new, increased price--all other values constant--into the equation which describes economic rent:  $(R=E(p-a)-Efk)$ . The new values for economic rent which would result would lead to a new rent gradient line of identical slope as before, but with an increased elevation. Figure 5 shows the new rent gradient ( $A_{t+1}$ ) and illustrates how the new spatial pattern of production evolves by extension of the outer margin.

#### Interdependence of Space-Price Equilibrium Points

The expansion in activity A occurred by decision-makers who formerly

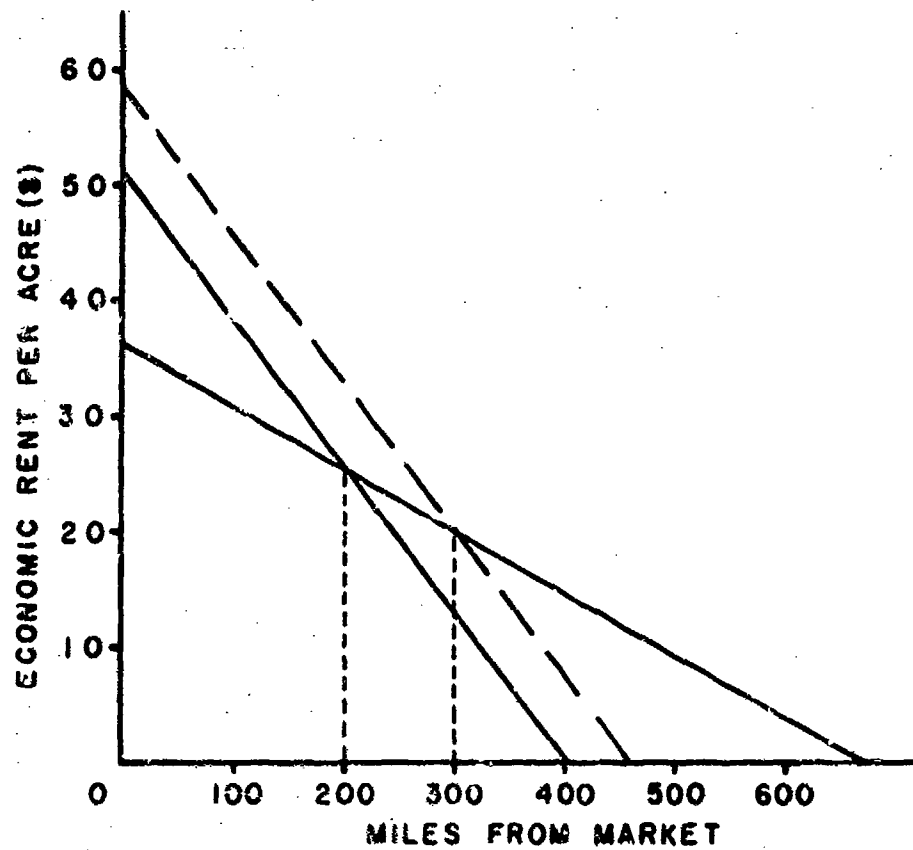


Figure 5: Change in economic rent of A following an increase in market price for A

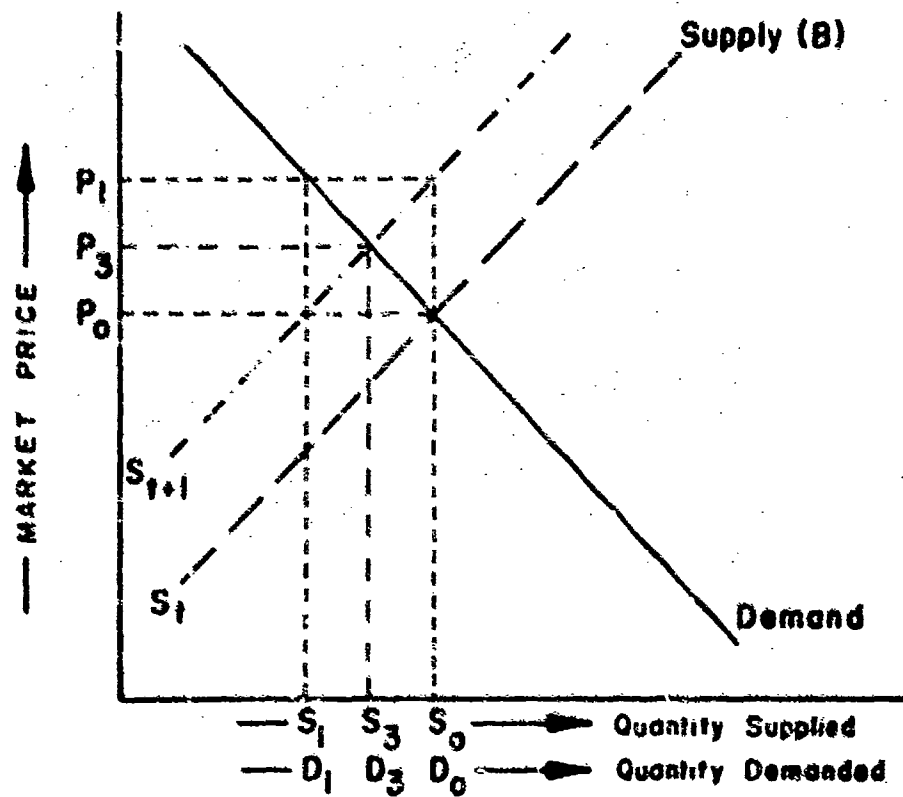


Figure 6: Change in space-price equilibrium of activity B following the consequences of an increase in price of product A

produced activity B and who switched to producing A. The supply of activity B at its old price must therefore be reduced. In other words, if activity B were at a space-price equilibrium point before the change in price of A, it will no longer be in equilibrium after the consequences of this price increase have had their effect on the spatial pattern of production. Figure 6 shows how the new equilibrium point for activity B will be found. Under the prevailing equilibrium price for B ( $P_0$ ), the amounts supplied ( $S_0$ ) and demanded ( $D_0$ ) are identical. Following the spatial adjustments shown in Figure 5 there is necessarily a new supply curve for activity B: i.e.,  $S_{t+1}$  in Figure 6. The similarity between Figures 4 and 6 should be noted. The process of reaching a new equilibrium point is identical even though the cause of the disturbance postulated in Figure 4 is quite different from the cause of the disturbance in Figure 6. The figure shows that after the spatial expansion of activity A,  $S_1$  of B would be supplied at the previous price of  $P_0$ , whilst a price of  $P_1$  -- higher than  $P_0$  -- would still result in all the new production of B being consumed. The new price  $P_1$  would not, however, be an equilibrium price since it would result in the old level of supply ( $S_0$ ) being maintained when the demand at the new price would be insufficient for that level of production. The equilibrium point would be  $P_2$  which would be between the old price of  $P_0$  and  $P_1$ .

#### Spatial Interdependence of Activities

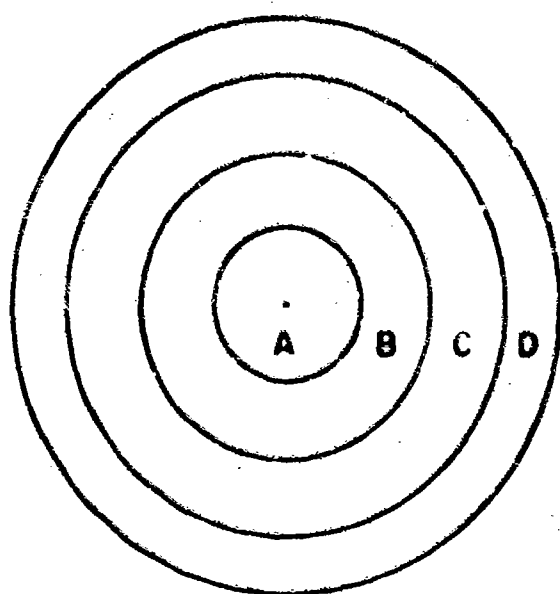
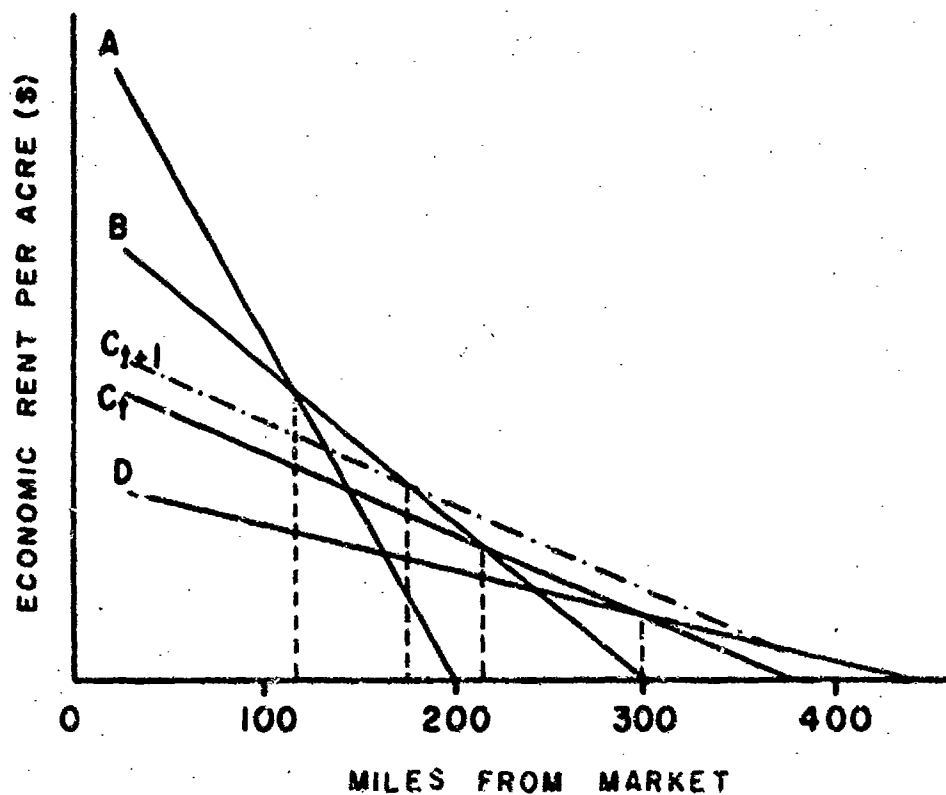
This cannot be the end of the analysis for the re-adjustment in the production of A following an increase in demand for it and the consequent increase in its price were deduced on the basis that all other variables would

remain constant. But it has just been shown that one of the effects of the expansion in production of A would be an increase in the price of B. An increase in the price of B leads to an increase in the elevation of its economic rent curve, just as an increase in the price of A had previously led to the increase in elevation of the rent gradient for A (Figure 5). This increase in the elevation of B would push back some of the spatial gains made by activity A. Now the spatial pattern for activity A is once again out of step with its space-price equilibrium and re-adjustments are brought about which once again will affect the pattern of production of other activities.

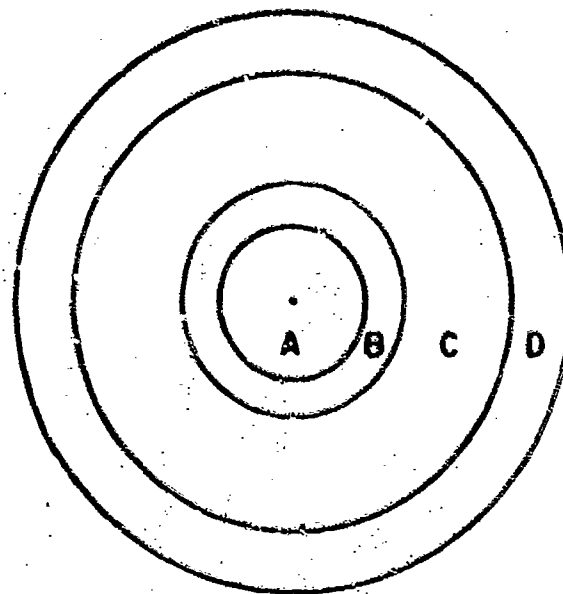
In this simplified hypothetical world we have demonstrated the meaning of space-price equilibrium and also have demonstrated why we must regard the spatial pattern of production as an expression of the simultaneous spatial solution of all space-price equilibrium points. In the real world we might expect disturbances to continually distort the time equilibrium pattern but we should understand the nature of the spatial equilibrium production pattern before we examine the effects of these distortions in more detail.

#### Spatial Interdependence with Many Activities

A world with only two activities is a very much simplified world. It is easy to see, though, that the conclusions derived in the analysis above are in no way restricted to a two-product world. In Figure 7 rent gradients for four activities are shown with a hypothetical change in elevation for activity C. Spatial expansion in this activity involves simultaneous reduction in the space for both activity B and activity D. In the second phase of the re-adjustment process both these activities will experience a rise in the elevation



SPATIAL PATTERN BEFORE  
PRICE INCREASE ( $C_1$ )



SPATIAL PATTERN AFTER  
PRICE INCREASE ( $C_{1+1}$ )

Figure 7: Changes in patterns of production: a four-product example with increase in price of product C

of their rent gradient leading to an expansion of the agricultural frontier in the case of D and a reduction in the space devoted to activity A in the case of B.

#### The Process of Spatial Re-Adjustment

As we have seen the process of spatial re-adjustment which follows from only one disturbance is intricate. It is a process in which the effects of changes in one activity pattern affects a second activity pattern in such a way that the changes induced in it are themselves the causes of further changes in the first activity. Stated more briefly we may say that it is a process in which A affects B which in turn affects A and so on. The successive adjustments in this cyclical process become smaller and smaller as all activities approach their space-price equilibrium point. However, this cycle of causation is almost certainly never completed since before one disturbance runs its course out, some new one will most likely have entered the system. Indeed, many disturbances are, by their very nature, continuous through time and therefore a spatial system in equilibrium is more likely to be an exception than a rule.

#### A Computerized Model for Spatially Variable Environments

In this model,<sup>10</sup> the variables in equation (1) are described as simple linear trend surface functions of change with respect to one location in the case of crop yields, and with respect to a linear feature in the case of production costs. Prices vary between markets. The model computes the values of R for a checkerboard sample of locations and prints a map showing the expected distribution of activities.

Figure 8 is an extremely simple case where neither yields nor production costs nor transport freight rates vary through the map area (see coding sheet data on Table 1). Yields are different for the different activities as also are the market prices. The zonal arrangement of land uses is the result. In Fig. 9, yields for each activity vary with distance from the center of the map and in Fig. 10 the rate of decline in yields is increased. In the two cases one can observe the progressive distortion of the zonal pattern of Fig. 8 resulting from the inclusion of spatial variability in yields. In Fig. 11, to the data of Fig. 8 is added the locations of five new markets that purchase one of the four commodities produced in the area. The degree of intrusion of these new markets on the previous land use pattern is indicative of the degree of competition between the activity and other activities that can be marketed in the two original markets.

Table 2 shows the input data for Figures 12 through 15. Figures 12 and 13 portray an area without transport costs to market but with yields of activities varying systematically with distance from the white squares within the map areas. This simple case produces the "proximal" map with activity areas as dirichlet regions with reference to the locations of highest yield. In Figure 13, the rate of decline in yields is greater per unit distance for activities one and four (see legend in Table 2). In Figure 14 transportation costs are introduced to the two markets and in Figure 15 production costs are varied with distance from the river. The similarity of land use patterns of these two figures despite the obviously different causes involved, may be seen as a warning of the dangers of inferring causes directly from land use patterns.



Table 1.  
Simulation of Land Use Patterns  
Coding Sheet for Solup (Version 1A)

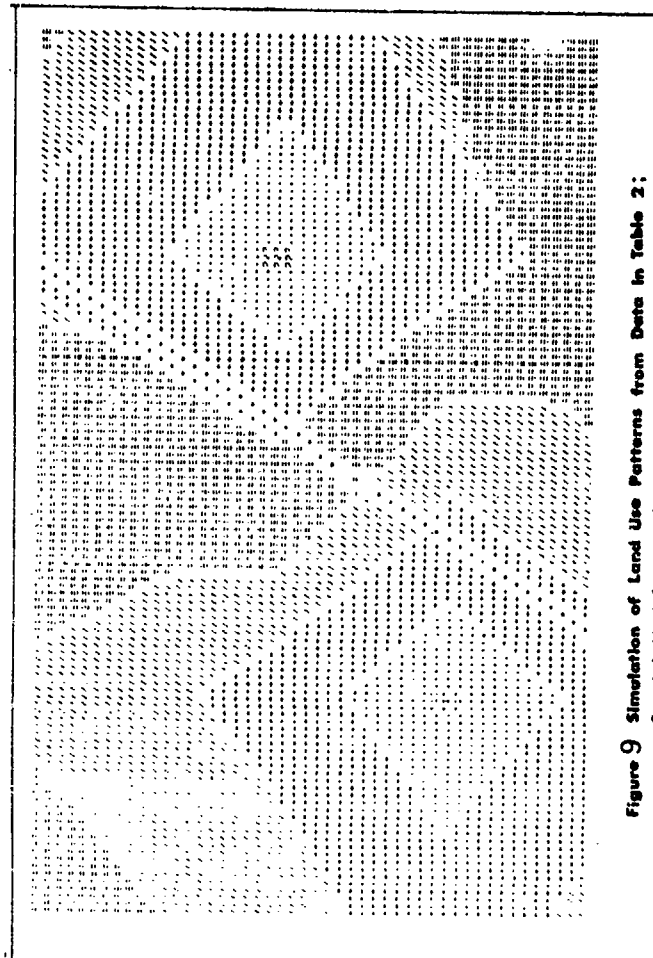


Figure 9 Simulation of Land Use Patterns from Data in Table 2:  
Spatial Variability of Yields

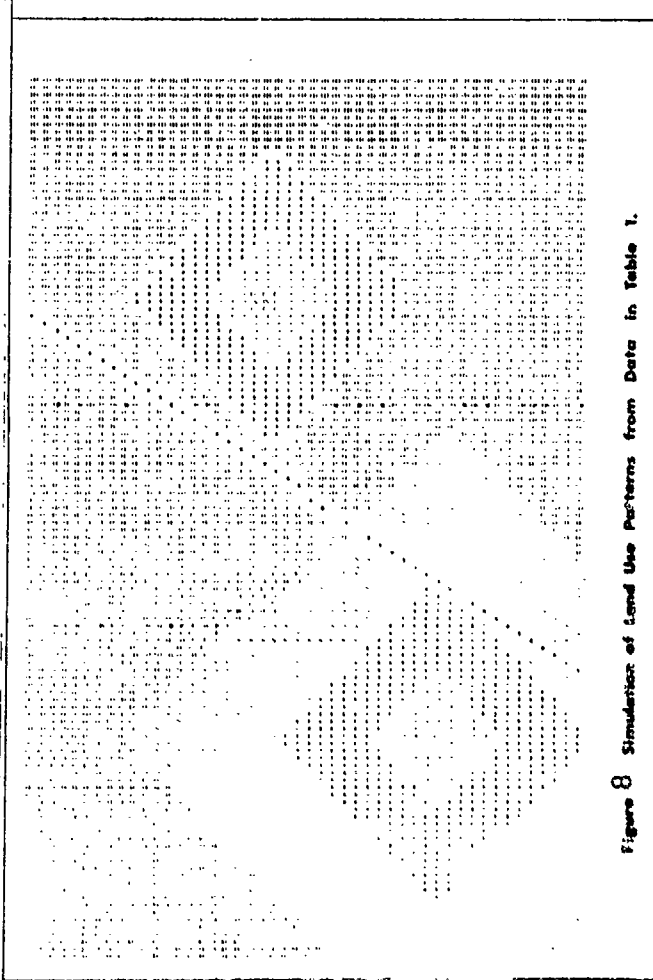


Figure 8 Simulation of Land Use Patterns from Data in Table 1.

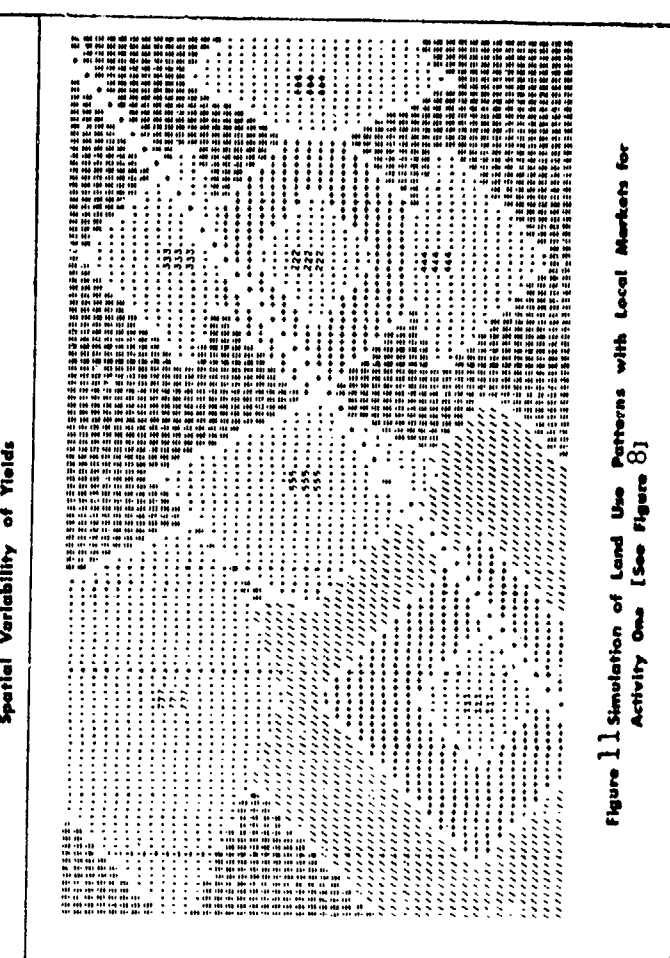


Figure 11 Simulation of Land Use Patterns with Local Markets for  
Activity One [See Figure 8]

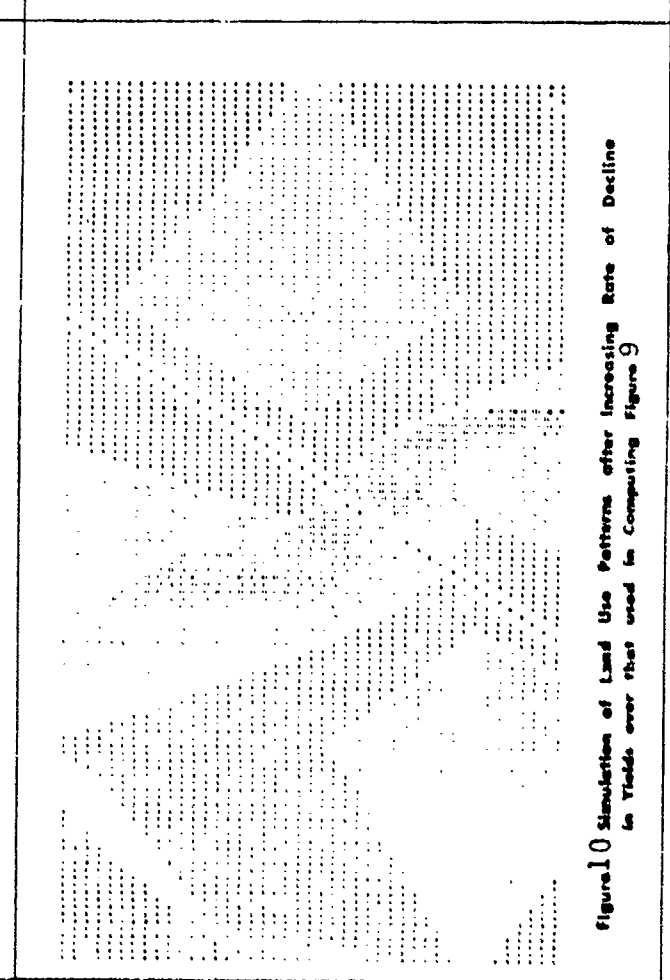


Figure 10 Simulation of Land Use Patterns after Increasing Rate of Decline  
in Yields over that used in Computing Figure 9

Figure 12 Land Use Patterns with Yields Varying Through Space and no Transport Costs

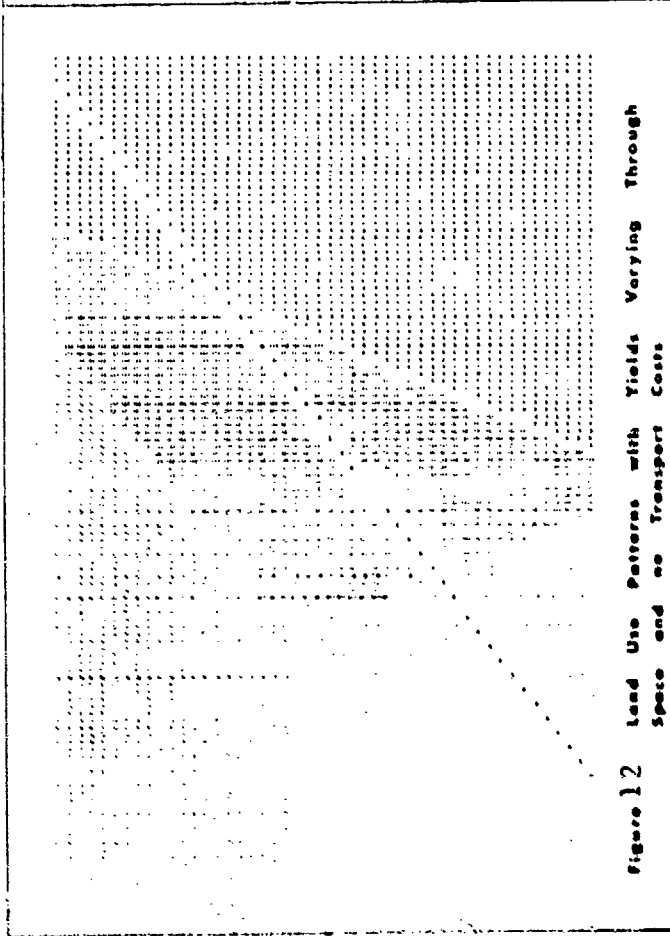


Figure 13 Effect of Increasing the Rate of Decline in Yields for Two of the Four Activities

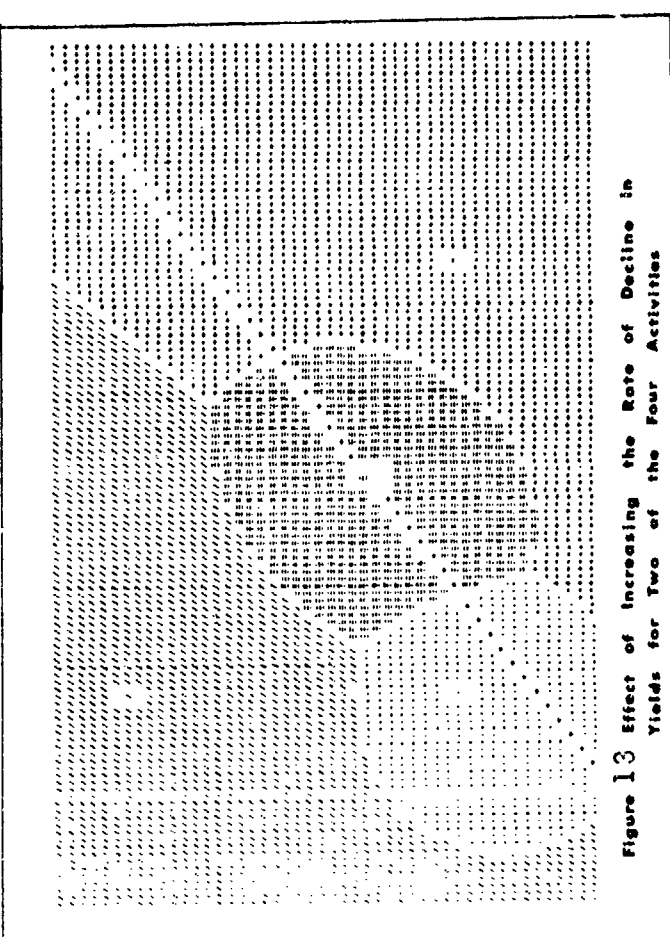


Figure 14 Transportation Costs to Two Markets with Equal Rates of Decline in Yields

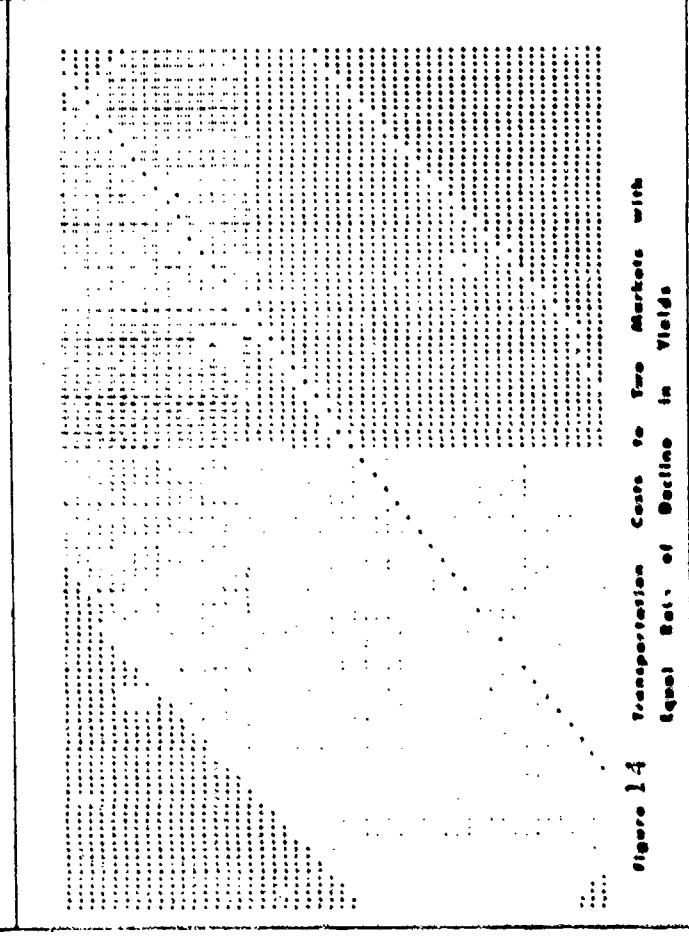


Figure 15 Production Costs Increasing with Distance from the River

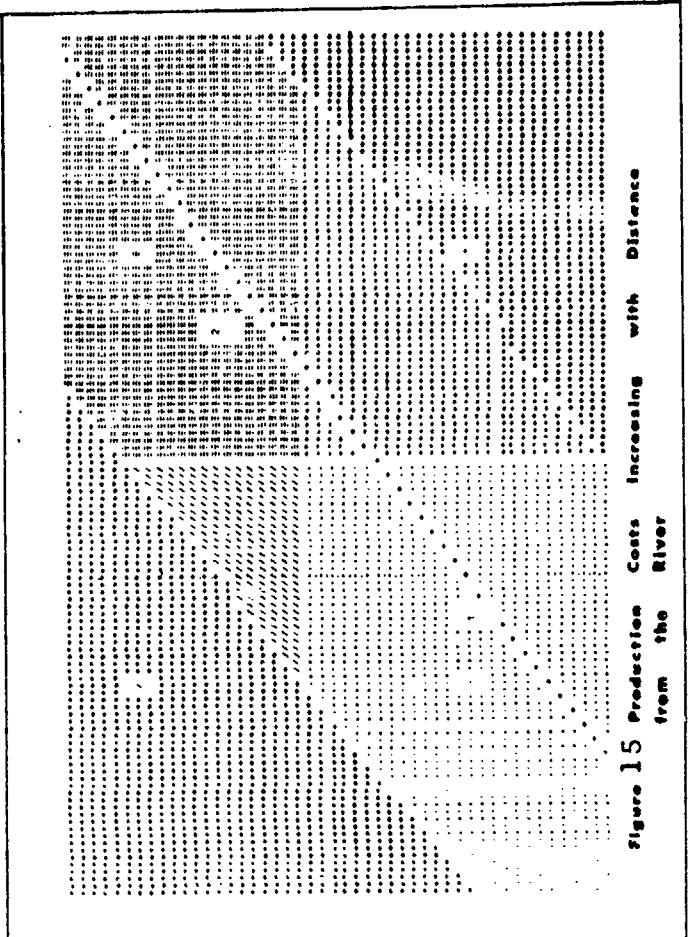


Table 2: Input Data for Figures 12 through 15

FIGURE 12

Market Prices	Markets 1 ....	Activities			
		1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
Production Costs					
At the River		50.00	50.00	50.00	50.00
Rate of Change from River		0.00	0.00	0.00	0.00
Transport Rate Functions					
Initial Cost		0.00	0.00	0.00	0.00
Transport Rate		0.00	0.00	0.00	0.00
Activity Yield					
Optim. Loc.		120.00	120.00	120.00	120.00
Gradient for Yield Decline		0.10	0.10	0.10	0.10

FIGURE 13

Market Prices	Markets 1 ....	Activities			
		1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
Production Costs					
At the River		50.00	50.00	50.00	50.00
Rate of Change from River		0.00	0.00	0.00	0.00
Transport Rate Functions					
Initial Cost		0.00	0.00	0.00	0.00
Transport Rate		0.00	0.00	0.00	0.00
Activity Yield					
Optim. Loc.		130.00	120.00	120.00	130.00
Gradient for Yield Decline		0.30	0.10	0.10	0.30

LEGEND	Activities			
	1	2	3	4
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....

where \*\*\*\* Appears on Map, All Activities have Negative Rent

Table 2: Input Data for Figures 12 through 15

FIGURE 14

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.00	0.00	0.00	0.00
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.10	0.10	0.10	0.10
Activity Yield					
	Optim. Loc.	130.00	125.00	120.00	115.00
	Gradient for Yield Decline	0.10	0.10	0.10	0.10

FIGURE 15

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.10	0.10	0.10	0.10
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.20	0.20	0.20	0.20

	1	2	3	4
.....	.....	+++++	/////	.....
LEGEND	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....
.....	.....	+++++	/////	.....

Where \*\*\*\* Appears on Map, All Activities Have Negative Rent

Table 3: Input Data for Figure 16 through 19

FIGURE 16

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.00	0.00	0.00	0.00
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.10	0.10	0.10	0.10
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield	0.05	0.10	0.15	0.20

FIGURE 17

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.00	0.00	0.00	0.00
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.10	0.10	0.10	0.10
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield	0.20	0.15	0.10	0.05

LEGEND

.....	+++++	/////	100 100 100 100
.....	+++++	/////	100 100 100 100
.....	+++++	/////	100 100 100 100
.....	+++++	/////	100 100 100 100
.....	+++++	/////	100 100 100 100
.....	+++++	/////	100 100 100 100

Where \*\*\*\* Appears on Map, All Activities Have Negative Rent

Table 3: Input Data for Figures 16 through 19

FIGURE 18

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.00	0.00	0.00	0.00
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.10	0.10	0.10	0.10
Activity Yield					
	Optim. Loc.	130.00	125.00	120.00	115.00
	Gradient for Yield	0.20	0.15	0.10	0.05

FIGURE 19

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	60.00	55.00	50.00	45.00
	Rate of Change from River	0.10	0.10	0.10	0.10
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield	0.20	0.20	0.20	0.20

.....	+++++	/////	#####
.....	+++++	/////	#####
.....	+++++	/////	#####
.....	+++++	/////	#####
.....	+++++	/////	#####
.....	+++++	/////	#####

where \*\*\*\* Appears on Map, All Activities Have Negative Rent

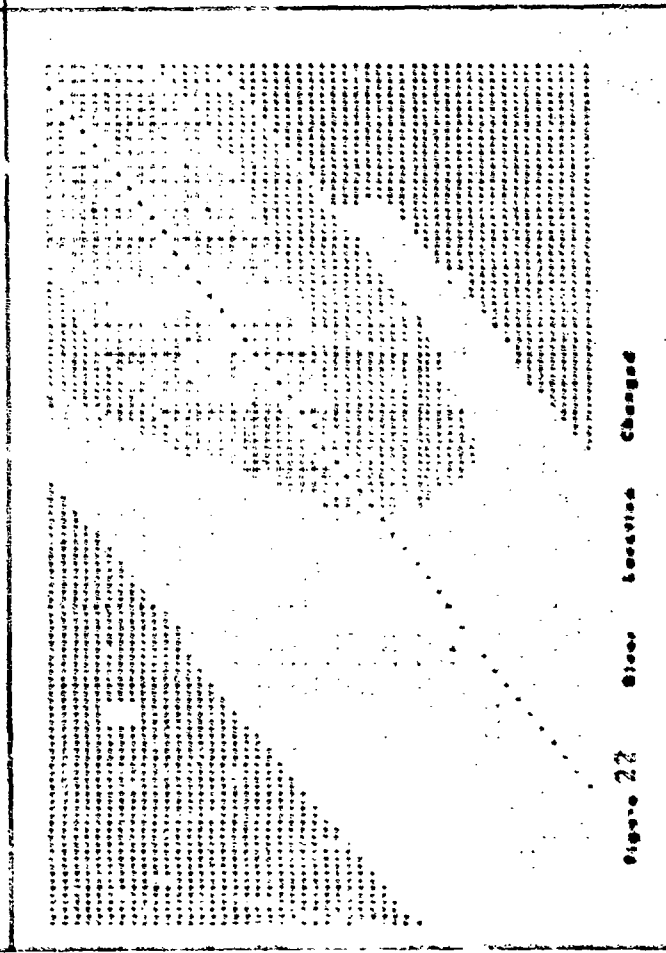
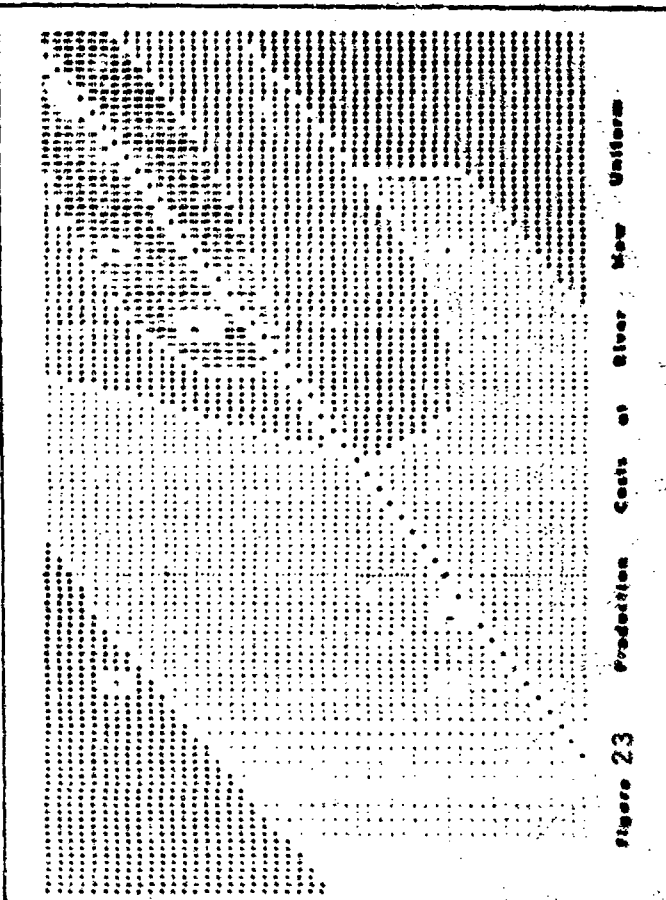
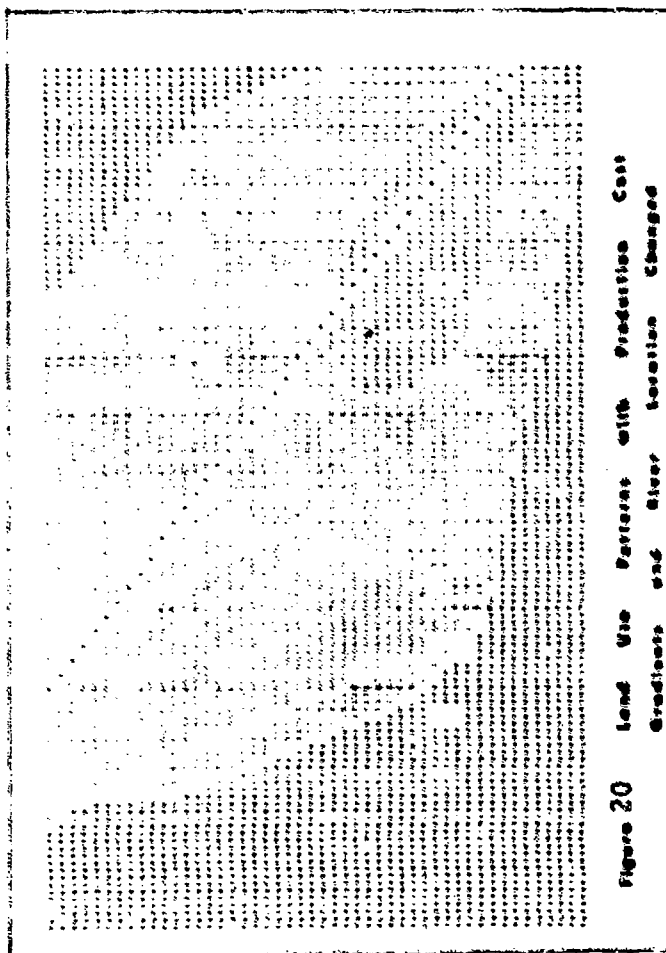
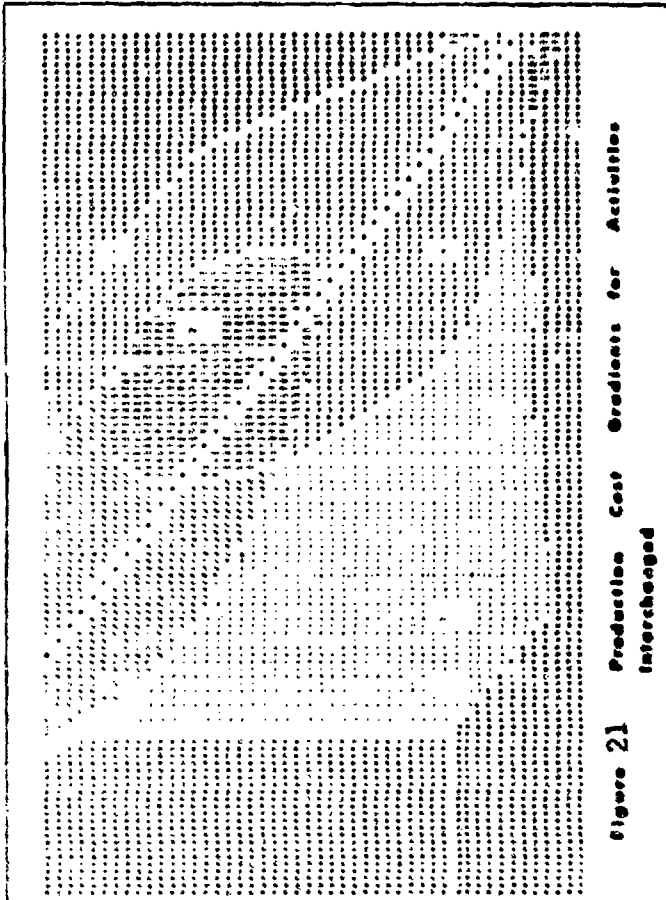




Table 4: Input Data for Figures 20 through 23

FIGURE 20

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	60.00	55.00	50.00	45.00
	Rate of Change from River	0.10	0.10	0.10	0.10
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.20	0.20	0.20	0.20

FIGURE 21

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	60.00	55.00	50.00	45.00
	Rate of Change from River	0.05	0.10	0.15	0.20
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.20	0.20	0.20	0.20

	1	2	3	4
.....	.....	+++++	/////	/////
.....	.....	+++++	/////	/////
.....	.....	+++++	/////	/////
.....	.....	+++++	/////	/////
.....	.....	+++++	/////	/////
.....	.....	+++++	/////	/////

LEGEND

Note: \*\*\*\* Appears on Map. All Activities Have Negative Rent

contd. Table 4

Table 4: Input Data for Figures 20 through 23

FIGURE 22

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	60.00	55.00	50.00	45.00
	Rate of Change from River	0.05	0.10	0.15	0.20
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.20	0.20	0.20	0.20

FIGURE 23

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	60.00	55.00	50.00	45.00
	Rate of Change from River	0.05	0.10	0.15	0.20
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.20	0.20	0.20	0.20

LEGEND

1	2	3	4
.....	.....	.....	.....
.....	.....	.....	.....
.....	.....	.....	.....
.....	.....	.....	.....
.....	.....	.....	.....
.....	.....	.....	.....
.....	.....	.....	.....

Where \*\*\*\* Appears on Map, All Activities Have Negative Rent

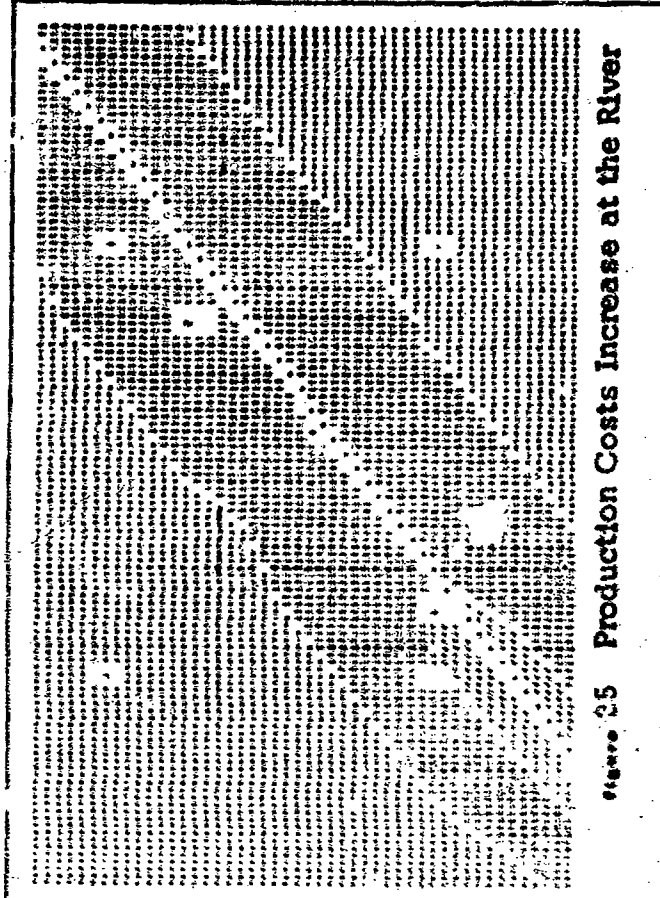


Figure 25 Production Costs Increase at the River

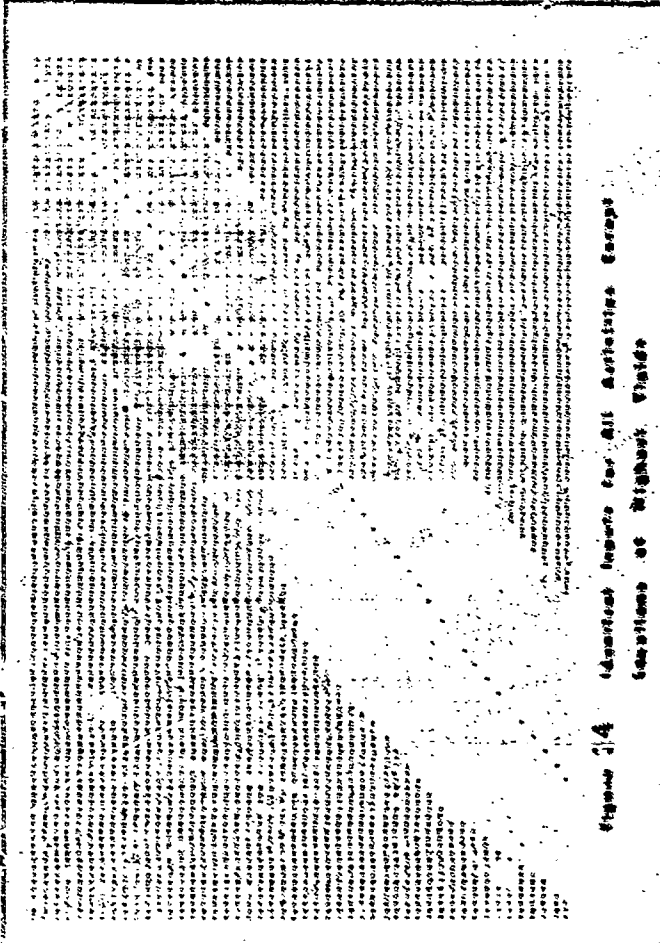


Figure 26a Member Price Differences, Western Production Costs

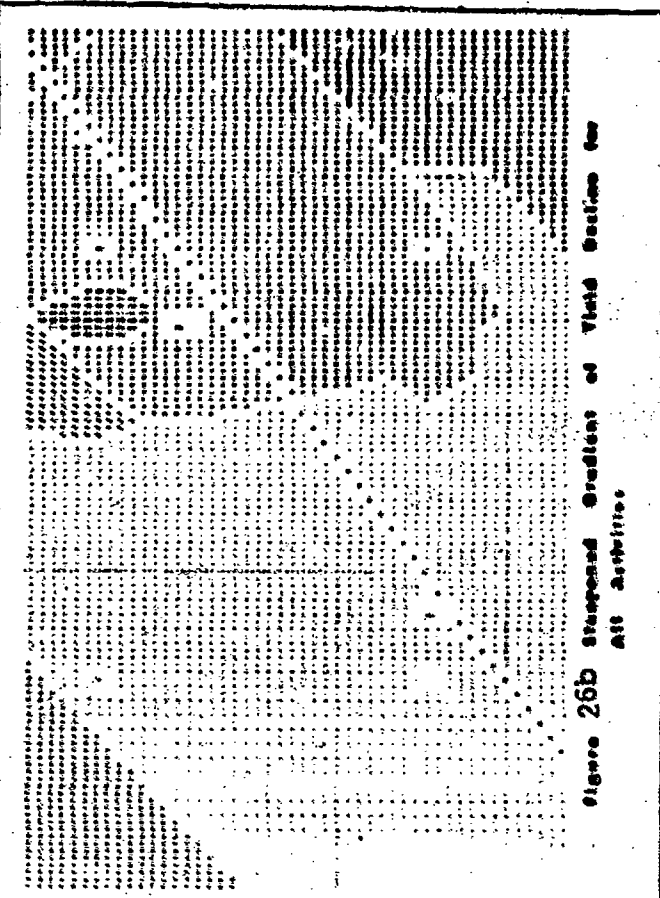


Figure 26b Steepened Gradient of Yield Section for All Activities

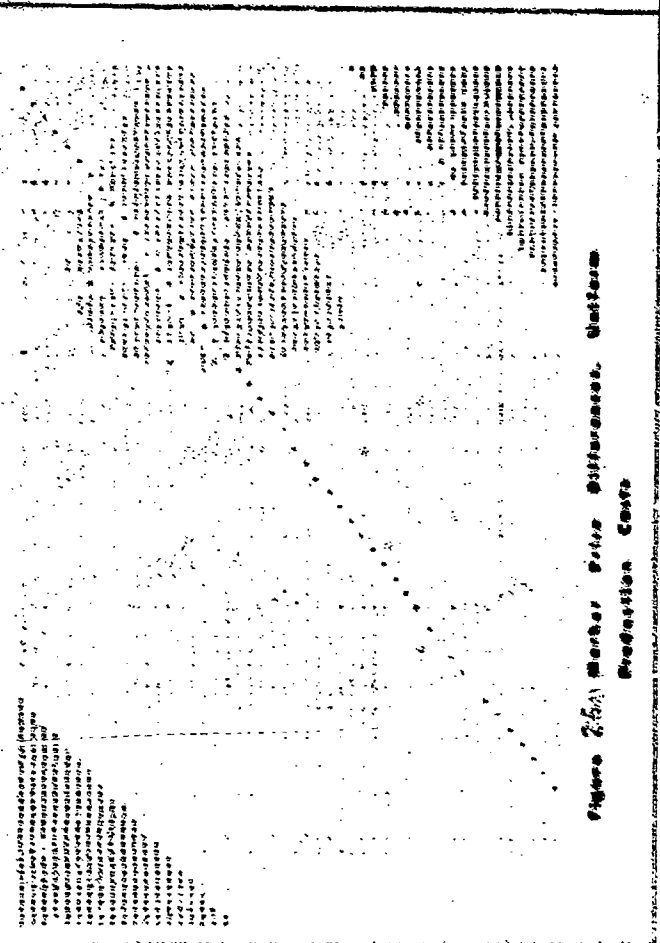


Figure 26c Member Price Differences, Western Production Costs

Table 5: Input Data for Figures 24 through 26b

FIGURE 24

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.20	0.20	0.20	0.20
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.10	0.10	0.10	0.10

FIGURE 25

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	100.00	100.00	100.00	100.00
	2 ....	100.00	100.00	100.00	100.00
Production Costs					
	At the River	70.00	50.00	50.00	50.00
	Rate of Change from River	0.20	0.20	0.20	0.20
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.05	0.05	0.05	0.05
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.10	0.10	0.1	0.10

LEGEND

1	2	3	4
.....	*****	//////	.....
.....	*****	//////	.....
.....	*****	//////	.....
.....	*****	//////	.....
.....	*****	//////	.....
.....	*****	//////	.....

Where \*\*\* Appears on Map, All Activities have Negative Rent

contd. Table 5

Table 5: Input Data for Figures 24 through 26b

FIGURE 26a

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	110.00	100.00	90.00	80.00
	2 ...	110.00	100.00	90.00	80.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.00	0.00	0.00	0.00
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.10	0.10	0.10	0.10
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.10	0.10	0.10	0.10

FIGURE 26b

Market Prices		Activities			
	Markets	1	2	3	4
	1 ....	110.00	100.00	90.00	80.00
	2 ....	110.00	100.00	90.00	80.00
Production Costs					
	At the River	50.00	50.00	50.00	50.00
	Rate of Change from River	0.00	0.00	0.00	0.00
Transport Rate Functions					
	Initial Cost	5.00	5.00	5.00	5.00
	Transport Rate	0.10	0.10	0.10	0.10
Activity Yield					
	Optim. Loc.	120.00	120.00	120.00	120.00
	Gradient for Yield Decline	0.20	0.20	0.20	0.20

LEGEND	.....	+++++	/////	=====
	.....	+++++	/////	=====
	.....	+++++	/////	=====
	.....	+++++	/////	=====
	.....	+++++	/////	=====
	.....	+++++	/////	=====

Where \*\*\*\* Appears on Map, All Activities Have Negative Rent

supporting the conclusion that land use patterns are most sensitive to small spatial changes in relevant environmental characteristics. In Chapter II we saw how the early issues of the new journal Economic Geography in the 1920's published the works of O.E. Baker which emphasized the general principles of location of agricultural activities. In the above example we see how quantification became the vehicle whereby many of these principles became formalized and made operational with respect to the spatial patterns of relevant variables. This is a common first step in the quantifying of a theory which previously has been stated in verbal or graphical terms. A later step is the application of a model -- which is a quantitatively interpreted form of a theory -- to some realistic situation. Here the intent is to apply the model to realistic initial conditions and to deduce the expected consequences. Comparison is then made between these and reality to determine whether the theory yielded correct "predictions".

#### Comparisons of Derived Patterns with those Observed

Attempts to implement such models as that described above have usually floundered on the problem of data collection, data availability, and data analysis. That Thünen himself wished to move in this direction is clear from Johnson's critical analysis of the role of Thünen's circles in his work.<sup>11</sup>

Thünen envisioned a second, larger state where the diameter of the cultivated plain would reach from Calabria in the south to Jutland in the north, where height and fertility are the same, no navigable river exists, and where a railroad net covers the circular plain uniformly. Here, climate is to be the variable. Thünen wrote: "Thus we have the tableau into which we must enter our data. Nobody, least of all the author, is ready now to design such a painting since all data for it are missing."<sup>12</sup>

Though Johnson showed how Thünen had not intended the illustrations of land use patterns in his hypothetical area to be interpreted literally,<sup>13</sup> and indeed that errors in the famous diagram had been introduced so that all activities would appear in the octavo size of the printed edition, the literal interpretation of von Thünen's work continues.<sup>14</sup>

In the illustration described below, the original data from Der Isolierte Staat<sup>15</sup> has been input to the computer model used in the previous illustrations.<sup>16</sup> Employing a single market and uniform transportation rates, a common point of maximum yield (the market), and the assumption of only one marketable commodity for each activity, five activities were input to the model. The activities as they appear in Figure 27 are: 1) rye production by the improved system, 2) rye production by the three field system, 3) forestry, 4) vegetable-potato production, and 5) butter production. Conceptual and perceptual difficulties encountered in detailing each of the activities will be presented below.

The inner-most ring was forced to be present by establishing high yield and a high gradient for yield decline, (see Table 6). This measure was necessary because the inner ring is determined by the combination of manure hauling rates and prices, vegetable yields, prices and costs, as well as being closely determined by the perishability of the goods. It was not possible to derive a single price, yield, or production cost from the data presented in the book. This is one of the many cases encountered in which the popular misconception of the output of von Thünen's work being a few distinct concentric rings was seen to be a great over-simplification. It

Table 6: Data Input for Programmed von Thünen Model  
(Fig. 27)

Market Prices		Activities				
	Markets	1	2	3	4	5
	1 ....	36.00	36.00	15.50	40.00	245.00
Production Costs						
	At the River	13.70	11.75	1.04	2.00	51.00
	Rate of Change from River	0.0	0.0	0.0	0.0	0.0
Transport Rate Functions						
	Initial Cost	1.00	1.00	1.00	1.00	1.00
	Transport Rate	0.69	0.69	0.71	5.00	0.83
Activity Yield						
	Optim. Loc.	132.00	114.00	240.00	300.00	1.00
	Gradient for Yield Decline	0.0	0.0	0.0	70.00	-0.10

Symbols Used in Activity Map		1	2	3	4	5
		Rye	Rye	Forestry	Veg-Potato	Butter
LEGEND	.....	++++++	////////	=====	&&&&&	
	.....	++++++	////////	=====	&&&&&	
	.....	++++++	////////	=====	&&&&&	
	.....	++++++	////////	=====	&&&&&	
	.....	++++++	////////	=====	&&&&&	
	.....	++++++	////////	=====	&&&&&	

Where \*\*\*\* Appears on Map, All Activities Have Negative Rent



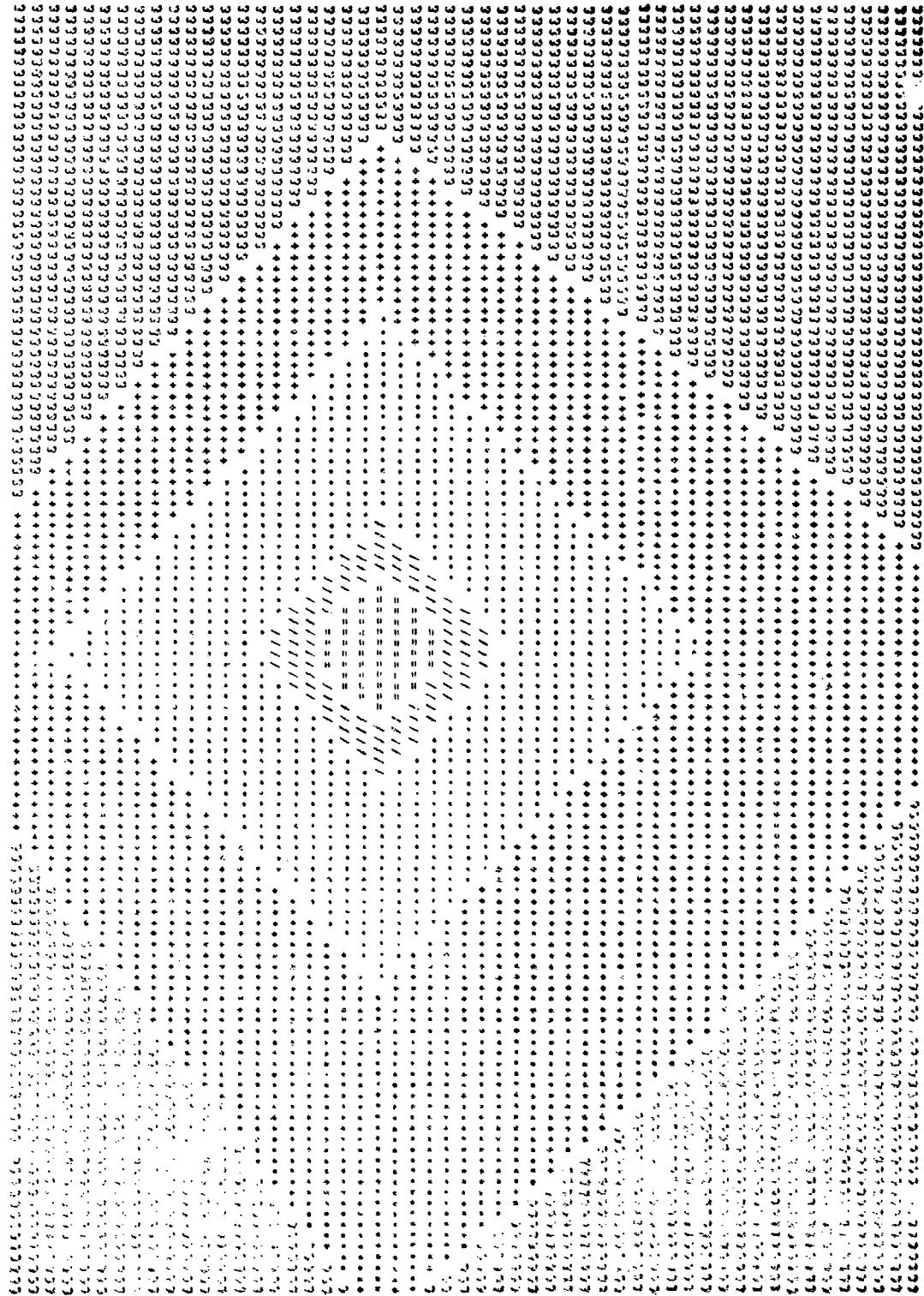


Figure 27: Recreation of von Thünen's Concentric Land Use Zones from Original Data

would be hypothetically possible to generate a separate ring for each vegetable, tuber, and dairy product.

The data for rye production are taken directly from the book<sup>17</sup> assuming a market price of 1.5 thalers per bushel, fertility of 8 bushel crops, and the "ideal" procedure for the two methods of farming. It should again be stressed that this represents an over-simplification of the pattern deduced by von Thünen. In fact, a continuum for rye farming from the pure improved system to the three field system with numerous possible modifications between is presented in the original work.

Forest production data are also taken directly from the text with the provision stated by von Thünen that price will be determined by the activities on either side of the forestry ring.

Butter production was calculated from the data in the text except for the difficulty presented due to the fact that the activity is functionally related to on-the-farm rye price. When rye is worth too little to ship to market, it is fed to cattle thereby raising butter yield. In order to simulate this increase in production outward from the center, a negative gradient of decline was assumed, (equivalent to an increase in yield with distance from the market). The calibration of this gradient was achieved through trial and error.

This illustration shows how the computer has revolutionized spatial analysis. Related to the actual data used by the nineteenth century originator of location theory, the example shows how contemporary quantitative geography is released from the drudgery of repetitive computation for

new locations in which, though the formulas developed remain unchanged, the values of the variables often vary in a capricious manner through earth space so that no simple spatial functional form for the activity pattern can be deduced. In this sense, each spatial pattern is unique as a morphological pattern. Only when the spatial pattern is regarded as the outcome of a decision-making process applied to the values of the variables, is generality achieved. In such circumstances, analysts of spatial form not unexpectedly, find difficulty in identifying spatial (morphological) generalizations.

A study by Wolpert<sup>18</sup> made observations on the farming decisions and resource availability of a sample of Swedish farmers and computed the optimum return that each sample farmer could have achieved by using his resources differently. The optimum productivity values were determined by means of a linear programming analysis<sup>19</sup> for representative farm situations with interpolation of the results to the circumstances of each sample farm. A comparison of actual productivity with optimum productivity "revealed that the average farmer achieved only two-thirds of the potential productivity which his resources would allow."<sup>20</sup> Wolpert concluded that rather than regarding men as optimizers, they should be regarded as searching for satisfactory outcomes and, he argued, they might better be called "satisficers".<sup>21</sup> Further study by Wolpert indicated that regional variations existed in the amount of knowledge possessed by the sample farmers and that variability in potential productivity due to vagaries in both climatic and economic factors might lead the sample population to diversify their activity even at the risk of incurring smaller profits. However, these differences did not appear strong

enough to explain the large discrepancy between potential and actual productivity on the sample farms and this led Wolpert to conclude that:

The concept of the spatial satisficer appears more descriptively accurate of the behavioral pattern of the sample population than the normative concept of Economic Man. The individual is adaptively or intendedly rational rather than omnisciently rational.<sup>22</sup>

Wolpert's study was a sophisticated quantitative analysis at the level of the individual decision-maker. It was influential in a number of respects in that it focused attention on the applicability to classical geographic problems of decision-making models developed outside of geography; it illustrated the utility and power of mathematical programming analysis within the discipline; it focused attention on particular spatial biases in behavior deviation from model-norms; and it connected spatial biases in the communication network with variations in behavior patterns. In short, Wolpert's originality in that work was to combine place specific variables of the environment with a behavioral, decision-making process to derive a spatial configuration. In this respect his work, though widely cited, has been rarely emulated in its most fundamental characteristics.

#### Comparative Survey of Major Location Theories circa 1955

Discussion of the impact of quantification on location theory should acknowledge the state of development of location theory in the mid-1950's. The first impact was undoubtedly the revival of interest -- which had not been great in the previous two decades -- in the theories. However, after the middle 1950's a strong interest developed in these theories for, as

discussed elsewhere, (Chapter II) the early quantifiers of this period frequently emphasized the relationship between quantification of a discipline and theory development. This interest however, far from being an integrated approach to theory development, took the form of quite separate approaches. First however, we will consider the state of those theories in the mid-50's. The four major location theories can be described in terms of the four elements of the methodological approach with which this chapter began, (Table 7). There was an early recognition that these theories would only satisfactorily relate to real world conditions when the simplifying statements of the first three elements were made more realistic. Schaefer's<sup>23</sup> methodological statement had, however, described a different kind of theory than this classical theory. The image of the nature of geographical theory that he had conveyed was of a body of empirically derived spatial generalizations. Insofar as these were derived from the classical theory, they originated from row four of Table 7: that is, from the theorems of classical theory. One such inductive approach, areal association, has been discussed in Chapter II. As was noted there, some criticisms of this approach referred to the fact that the approach was, in most of its applications, aspatial in the sense that spatial arrangement was not explicitly studied. This, of course, related to a second element of Schaefer's methodological approach which was that geographic theory should be a body of empirically tested generalizations relating to spatial arrangement. It is here that a connection with classical location theory was attempted. The connection between these two quite different groups of literature each using quantification--- the older

Table 7: Characteristics of Classical Location Theories

	Central Place Theory	Industrial Location Theory	Agricultural Location Theory	Spatial Diffusion Theory
1. Initial Conditions	Homogeneous plain uniform transport surface	Locations of markets, and raw material sites given; actual transport system	One market, homogeneous plain, uniform transport surface, no variations in factor costs	Distribution of opportunities for acceptance; uniform transport surface
2. Behavioral Postulates	Entrepreneur: will enter system when threshold purchasing power exists and will leave when this no longer exists. Consumer: will patronize nearest center offering the good i.e. distance minimization	Entrepreneur will locate at point of minimum total transfer cost for assembling raw materials and distributing final product	Farmer will engage in that activity which yields highest economic returns and will transfer in or out of given activity accordingly	Number of acceptances directly proportional to number of opportunities and inversely related to expected rate of interaction
3. Constraining Conditions	Hierarchical bundling of goods, (Christaller); no excess profits, (Lösch)	Constant returns to scale and no substitutability between factors of production	Closed local system prices known	Symmetrical mean information field
4. Theorems	Triangular lattice of service points with hierarchy of services (Christaller), and mixed grouping (Lösch)	Orientations of production centers to raw material sites or market sites	Concentric (zonal) arrangement of land uses	A spatial pattern simulated by Markov chain process or by monte-carlo techniques

quantitative literature emphasizing mathematical deductive approaches from behavioral premises; the newer quantitative literature emphasizing mathematical induction from given spatial patterns, existed only in the opportunity afforded by the spatial theorems in the older literature to be tested by the developing methodology of both descriptive and inferential spatial statistics.<sup>24</sup>

It is this area, therefore, that most of the quantitative literature in the period 1955-1965 is to be found.

The early location theories, however, differed in the degree to which each was conducive to being tested by spatial statistics. Indeed it is surely interesting that the measure to which these theories received attention in the quantitative literature of the decade from 1955 on, appears to correspond with the degree to which each was conducive to being tested in this manner. Thus, agricultural location theory as developed by von Thünen<sup>25</sup> produced spatial patterns of a continuous nature over space whereas most of the early developments in spatial statistics were concerned with the analysis of discrete (point) patterns. Perhaps more fundamental is the fact that the spatial theorems of von Thünen had no relation to any given earth space and therefore could not be tested by spatial statistics except in unusual circumstances.<sup>26</sup>

Similarly, Industrial Location Theory (see Table 7) could be used to generate expected locational patterns only through a difficult process of data gathering and analysis. Perhaps not surprisingly, examples of such quantitative empirical studies were few.<sup>27</sup> Likewise with Diffusion Theory, model calibration and spatial simulations were so time consuming that implementation

had to wait for developments in computer analysis. Central Place Theory in contrast, seemed eminently suitable as the theoretical basis of quantitative studies of spatial structure. Although it was rejected by some for its simplifying assumptions about the initial conditions and about the form of behavior in the system,<sup>28</sup> virtually every form of spatial analysis used in geography<sup>29</sup> was applied at some time or another toward the recognition of central place patterns. For a time, such work became virtually synonymous with theoretical geography.<sup>30</sup> Thus our conclusion is that the work of the early quantifiers recognized the importance of building a body of geographic theory but that in the early period of quantification (1955-1963), the work completed either neglected traditional theory in its search for empirical generalizations; or it selected those aspects of location theory that referred to explicit spatial structures in conditions that were felt to be comparable to some areas on the earth. For such spatial structures, tests were devised to determine whether significant features from the theorems were to be found in the study areas.<sup>31</sup>

#### Influence of the Quantitative School 1964-1971

As quantification expanded into more parts of the discipline a variety of viewpoints and associated types of work related to location theory began to emerge.

#### The Normative School

This school of quantifiers were impressed with the possibility of adapting mathematical models developed in Operations Research to the



solution of locational problems. Much of economic location theory had also been founded on the premise that whichever locational pattern was most efficient is the one that should exist. Thus algorithms were developed or mathematical programming techniques applied to a range of problems that had previously been considered by classical location theory.<sup>32</sup> For example, Kuhn and Kuenne<sup>33</sup> developed an efficient algorithm for determining the least total transfer cost location for constructing an industrial plant that used fixed proportions of raw materials from various locations and served markets located across some defined region. Elsewhere, an algorithm was developed for the simultaneous location of several production sites.<sup>34</sup> Yeates<sup>35</sup> used the transportation solution of linear programming to locate school boundaries so as to minimize total travel distance subject to the constraint that schools had different capacities and Egbert, Heady and Brokken<sup>36</sup> used linear programming to determine which regions should go out of production for selected grains if total demand were to be met at the various markets from the most efficient producing areas. Day and Tinney<sup>37</sup> developed a programmed solution of the space-price equilibrium problem (discussed earlier in this chapter) and in so doing showed that space-price activity equilibrium could be reached without assuming advanced knowledge of future prices by farmers--a postulate of conventional theory that is clearly contrary to experience.

In industrial location studies, Casetti applied programming techniques to evaluate the different potentials of various locations in Canada for a new Iron and Steel Mill.<sup>38</sup> More recently, Scott has adapted dynamic Integer

programming to the problem of determining an optimal location of facilities to serve a given population and for the optimal sequencing of the location decisions. Elsewhere, he has reviewed the large literature which applies programming approaches to the location-allocation problem.<sup>39</sup> The literature in this area continues to expand fast, and given the predilection of researchers to address themselves to specific location problems, this trend is likely to continue.<sup>40</sup>

#### The Structural-Probabilistic School

A second group of quantifiers emphasized the stochastic nature of most behavioral processes and the inability of the researcher to trace back every influence affecting location decisions. Accordingly, reality was seen as the outcome of a probabilistic process operating over space which could be calibrated from historical data in the area in question rather like gravity models had traditionally been calibrated. One of the boldest attempts to model an urban system from this approach was the study by Morrill<sup>41</sup> in which three components of urban growth were treated consecutively over a small area of Sweden. Likelihood of industrial development was seen to be a function of distance from transportation route and of the pre-existing pattern of industrialization. From the historical data this component was calibrated. The chances for the development of a point as a central place was made a function of accessible population according to the classic logic of central place theory and was calibrated from the historic data. The migration of people in the area was modelled as a gravity model process according to conventional quantitative migration theory. For consecutive

periods these three components were collectively modelled in the study area in a monte-carlo simulation model. The outcome was a spatial pattern that was similar in many important respects to the historical data. Although this ambitious attempt to model the spatial elements of several aspects of a socio-economic system received many favorable reviews, examples of similar studies were few. The difficulties of model validation were present in that such models provide no unique outcome but rather a range of outcomes consistent with the stochastic nature of the models. The statement that reality was to be regarded as equivalent to one of the outcomes of the model--but not necessarily the outcome that occurs with the highest probability--left the researcher without criteria for adjudicating even different versions of the same model. Criterion for accepting such models was therefore the weak criterion of judging whether reality contained spatial-ordering principles that were not contained in the model outcome. Several techniques of spatial statistics (reviewed in Ch. IV) originated with this problem of identifying significant spatial structural features in any spatial distribution.<sup>42</sup> A further problem with such models (and many other types too) is that of identifying the degree of meaning variance in the models themselves.<sup>43</sup> When the models are calibrated with empirical data, the values of parameters are often seen to fluctuate greatly from one study area to another and from one time period to another in the same study area. With some models, this variability has been studied (see discussion of the gravity model below), however, in most of the simulation models of changes in economic or social activity through space, such problems were ignored.<sup>44</sup>

In Chapter II the influence of the gravity model on the work of the early quantifiers in geography was discussed. In this later period some work continued. An excellent review of the relationship between distance interaction and location theory through 1965 is found in the review by Olsson<sup>45</sup> and, specifically with reference to research in diffusion theory, in the later review by Brown.<sup>46</sup> Applications of the gravity model to the study of spatial interaction received comparatively little attention in geography although, in marketing its practical utility was recognized and it was extensively used.<sup>47</sup> In geography, however, the concept of distance-decay -- an offspring of the gravity model -- received attention in relation to more general efforts of location theory development. Thus distance-decay was central to the early work of Harvey<sup>48</sup> who attempted to prove that a distance-decay effect was operating in the determination of agricultural activity patterns. He showed that the degree of concentration in hop-growing in his study area in S.E. England was related to distance from the hearth region of hop-growing, notwithstanding the fact that all clearly measurable economic and physical variables that were related to the profitability of this form of enterprise did not particularly favor concentration in the hearth region. Harvey concluded that accessibility to the supporting services found in the hearth region appeared to be the primary explanation of this pattern centered on a point that could have easily existed elsewhere but which, for a variety of historical circumstances, happened to be located in mid-Kent.

In industrial location research, distance-decay functions were incorporated in the market-oriented study of Harris<sup>49</sup> and later in the work of Ray<sup>50</sup>

who showed how the location pattern of U.S. industrial subsidiaries in Canada could be modelled using the principle of a distance-decay rate from, in each case, the U.S. city with corporate headquarters to the cities of Canada. In central place studies, interaction models were used to predict the spatial form of tributary areas.<sup>51</sup>

An early study, which despite its importance appears to have remained unnoticed,<sup>52</sup> apart from a largely irrelevant criticism<sup>53</sup> investigated the systematic variation in the exponent parameter values of distance in the gravity model. This problem of parameter invariance was raised again much later by Marble and Nystuen<sup>54</sup> and by Morrill and Pitts.<sup>55</sup> Both studies concluded that, when other things are equal, the greater the density of opportunities for interaction, the greater the distance exponent in the gravity model after calibration with empirical data. The best fitting statistical curve for the normalized distance-decay rates received much attention for a time before it was recognized that no one functional form consistently fitted best empirical data.<sup>56</sup> In other words, researchers reconciled themselves to the notion that with gravity models meaning variance was high due to parameter instability.<sup>57</sup> This evidence led to the suggestion that spatial interaction might be more appropriately modelled by viewing spatial choice as the result of the application of a personalized space preference structure to a unique set of spatial alternatives, resulting in choice of the alternative with the highest preference.<sup>58</sup> Thus, identical preference structures could generate spatial choice patterns that would have variable parameter values when distance-decay models were calibrated to them.<sup>59</sup> This observation

led to the conclusion that the distance exponent on the gravity model may not be interpreted as a preference index--a "degree of friction of distance" as it is commonly referred to.<sup>60</sup> Further discussion of preference structure identification is found later in this chapter.

### Varying the Behavioral Conditions in Location Theories

#### Naivety of Current Postulates

There appears to be a tendency in all areas of social science to periodically question the usefulness of the postulates from which theories have been built. The most notable example is probably the debate in economics concerning the usefulness of the postulate of economic man on which so much of micro-economic theory is based.<sup>61</sup> Elsewhere, (Chapter III), we have discussed the epistemological issues involved in the search for "realistic" postulates in the construction of geographic theory. Here we focus on the effects of this argument on efforts to construct theory. The effect has been to dissipate the efforts of workers in this area in many quite different directions. Several such streams of effort are identified below.

#### The Normative Nature of the Postulates

Some emphasize that most behavioral location theories were never intended to be descriptively accurate but rather function as normative theories; prescriptive of what the world would be if people behaved according to the basic postulates of the theory. Those who are most true to this approach are no longer interested in the application of any theory they might develop to explain reality but rather become engrossed with the problem of

satisfying the optimality conditions they have specified. The emphasis on scientific rigor and the social engineering approach to social problems that frequently accompanies their work, draw this group into the pure locational analysis viewpoint. Some who acknowledge the normative nature of the postulates argue that they expect reality to approximate the theory. In Central Place Theory, for example, Lösch belonged to the former group and Christaller the latter group. Explaining that he wished to develop normative theory, Lösch concluded his work with:

"Life consists not only of development in time but of spatial diversity as well. Space stimulates the creative forces. And I see in my mind's eye an economic science that, more like architecture than like the history of architecture, creates rather than describes!"<sup>62</sup>

And with respect to explaining reality, he explained that the purpose of his empirical work was not to test his theory, but to test reality:

"No! Comparison now has to be drawn no longer to test the theory, but to test reality! Now it must be determined whether reality is rational. In any case, this, and not verification of theory, is the purpose of the following investigations.

"For the lack of confidence in reason it must not choose reality for its judge.... In a word, it must not become a science that describes chaos instead of preaching order."<sup>63</sup>

In contrast, Christaller concluded his book by claiming verification of his theory on empirical grounds.<sup>64</sup>

#### Rejection of the Postulates

Rejection of the postulates in any theory is followed by a variety of actions. An extreme case is a relapse into study of the descriptive characterization of individual areas; but, more commonly, alternative postulates

are offered<sup>65</sup> and debate commonly ensues as to whether the alternatives proposed are superior to those which they are intended to replace.<sup>66</sup> A second strategy in the face of the complexity of behavior is for the researcher to move the scale of the research to a more aggregative level and often to apply probabilistic allocation rules for behavior from data at this level. Much of Morrill's research follows this strategy.<sup>67</sup>

#### Restrict Evaluation to the Theorems

To some writers it has been self-evident that viable theories can only be built upon behavioral premises that correspond with observed behavioral patterns. Olsson and Gale, for example, in pointing out that traditional location theories in economic geography are extensions of the theory of the firm, and that the theory of the firm is built upon the two rationality assumptions that firms consistently seek to maximize profits and that they possess perfect knowledge, concluded that:

"It is only if these assumptions are fulfilled that traditional spatial theory is valid, and it is only then that it is meaningful to employ it in the computation of optimality and equilibrium solutions."<sup>68</sup>

However, M. Friedman, has argued that there is no merit in evaluating the "realism" of assumptions in a theory independently of the validity of predictions:

"Truly important and significant hypothesis will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be



explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained."

"To put this point less paradoxically, the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic", for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions."<sup>69</sup>

Thus, Friedman concludes that criticism of a theory based on a criticism of the falsity of its assumptions is:

"largely beside the point unless supplemented by evidence that a hypothesis differing in one or another of these respects from the theory being criticized yields better predictions for as wide a range of phenomena."<sup>70</sup>

As Cyert and March have pointed out; to Friedman, "The only crucial test of a theory is its predictive power."<sup>71</sup> While concurring with Friedman's refusal to discard unrealistic hypotheses, Cyert and Grunberg nevertheless argue that it is:

"...legitimate to keep unrealistic hypotheses only as long as nothing better is at hand. Such unrealistic hypotheses should be "retained" in the theoretical structure until a better hypothesis can be formulated. The situation is considered to call for a search for a more satisfactory hypothesis. In this context the fact that the disconfirmed hypothesis yields correct predictions for a given class of events must also be considered as a problem that requires solution."<sup>72</sup>

They argue further that a more "positive economics" would be one in which the "empirical laws of economics should be based on propositions of behavior that can be disconfirmed independently from the economic

situations."<sup>73</sup> Samuelson also took issue with Friedman in saying that:

"The fact that nothing is perfectly accurate should not be an excuse to relax our standards of scrutiny of the empirical validity that the propositions of economics do or do not possess."....."If the abstract models contain empirical falsities, we must jettison the models, not gloss over their inadequacies."<sup>74</sup>

The American Economic Association devoted a session to this issue in its 1962 annual meeting and invited Ernest Nagel to "referee" the argument. Nagel concluded that,<sup>75</sup> while he found Friedman's argument inconclusive, nevertheless he viewed his conclusion as sound.

Between the two extremes of those who hold that all behavioral postulates must be "realistic" and those who profess unconcern about the accuracy of postulates and who, rather, confine their interest to an evaluation of the predictive utility of a theory, are those who argue that the whole issue is irrelevant. They would argue that, although all decision-makers may not behave as postulated in the theory, in the long run only those who so behaved would survive, and therefore, the theory predicts the behavior of viable units.<sup>76</sup>

There are also those who argue that we must start from propositions with which we can deal analytically. They would argue that, even though it might be desirable to substitute other assumptions for those of conventional theory, it is not analytically possible to do so.

#### Independence of Behavioral Postulates

In a wide range of literature the point has been made that behavior in a spatial system and the spatial distribution of elements in that system are

inter-related. That is, spatial behavior varies according to the characteristics of the spatial system while those characteristics reflect, in part, the kind of spatial behavior that occurs in the system. Given this interdependence, it became clear that behavioral postulates from which theorems of spatial structure had been derived might inherently contain at the outset the deduced properties of the system to be derived. Curry<sup>77</sup> first pointed out this implication, though it is questionable whether he successfully avoided the problem in his work since a distance-decay function was the allocating mechanism for spatial interaction in the system he deduced, even though it is wellknown that the parameters of such functions vary with the characteristics of the spatial system in which they are calibrated. Elsewhere, we have reviewed other discussions of this issue,<sup>78</sup> and have noted that the problem of finding behavioral postulates independent of the spatial system they are intended to generate, is extremely difficult. The conclusion was that the first step in the direction of finding independent postulates was to make the distinction between "spatial choice" and "spatial-preference structure."<sup>79</sup> Postulates of spatial behavior that are direct generalizations of spatial choice patterns, most commonly will not be independent of the spatial system in which they were observed. However, it was argued that where spatial choice patterns have been analyzed to recover the underlying preference structure which, when applied to the local distribution of spatial alternatives generated the spatial choice patterns, such a preference structure is an admissible postulate for the construction of theory.<sup>80</sup> A later work clarified this point by showing that a hypothetical space-preference

structure that had been applied to a complex distribution of spatial alternatives to generate spatial choice patterns, could be successfully recovered from those choice patterns.<sup>81</sup> Since alternative preference structures could have generated different spatial choice patterns, the distinction between choice patterns and preference structures in spatial behavior was demonstrated. However, while a proof that these are different may clarify the situation, it is no proof of independence. The argument for independence rests on an intuitive level though some empirical corroboration will be outlined below. At the intuitive level is the notion of the impossibility of conceiving of a person moving from one area to another and making choices without having general rules (rather than place-specific rules) to guide his behavior. What are the origins of these rules? We may postulate a trial-and-error procedure whereby they are learnt in a given environment. Consequently, like most rules we may expect them to have a domain circumscribed by the range of experiences previously encountered. In the sense that this domain will not include all kinds of environments that may be encountered, such rules of behavior are not independent of environmental characteristics and it would be quite inappropriate to apply them to an environment outside of the original domain. Thus the apparent contradiction: we say that the preference structure is independent of the environment in which it is measured but that its use is restricted to environments in a particular domain. Interest should therefore center on the time and place restrictions that delimit this domain and thus the problem of independence of behavioral postulates is not a problem that is resolved by formulating a set of criteria to be used

to define independence. The problem therefore lies in defining the domain and in some respects this becomes a problem that can be solved only with reference to empirical knowledge. The goal is to discover the laws of human behavior relating to spatial choice that are neither space nor time specific and some criteria can be suggested to assist in reaching this goal. The problem, however, is a measurement one and will be discussed in section four below. At an empirical level, however, one attempt has been made to show that spatial choice patterns in two areas (Iowa and Michigan) although superficially different in terms of aggregate descriptions of behavior, can nevertheless be shown to have been generated by the same space-preference structures.<sup>82</sup> The inference here is thus that a behavioral postulate derived from Iowa would be appropriate for developing a theory of urban activity patterns (based on projected spatial interaction patterns) that would be applicable to Michigan. In a world-context, of course, a behavioral postulate applicable to the American mid-west may well prove dependent, in its application, to areas similar to the mid-west. Thus the domain of the spatial-interaction rules of this example appear to be the mid-west and all such areas. This is not a satisfactory definition, however, for it does not make clear what is meant by the mid-west nor, indeed, what is meant by "all such areas". The latter may be defined as area in which people react to the stimuli, as they are operationally defined in the preference structure, in the same way as in the area where the preference structure was originally measured. Furthermore, such areas must not contain stimuli to be evaluated that are not replicated in the source area.

An example may clarify this point. A preference structure for the Iowa rural population was used in an attempt to predict the trade area territories of towns in Saskatchewan. However, the attempt proved futile because it was soon found that many of the Saskatchewan rural population were so located with reference to service towns that they were choosing between towns at certain distances (the stimuli) that were never seriously considered in the Iowa context. Although such sized towns at these distances existed in Iowa, more satisfactory alternatives always existed, (owing to the greater density of towns in Iowa), so that the Iowa population never revealed how they would choose between such stimuli. Expressed differently, many of the Saskatchewan stimuli were outside the domain of the Iowa space-preference structure. The problem is one of "robustness". By how much may the environment change before "new stimuli" are encountered, rather than the situation where different environments simply mean different proportions of the various stimuli?

To this point independence of postulates has been discussed with reference to independence of the environmental characteristics that the deductive system containing the postulates will generate as theorems. But there is a second sense in which independence is required and that is with respect of one postulate to another and with respect to their mutual consistency.<sup>83</sup> McGill also summarizes this requirement in writing:

"The question of empirical validity aside, an adequate axiom system must be consistent: two contradictory consequences cannot be implied by the same system. A second important condition is that the axioms be independent, which means that no one axiom in a given system should be derivable from others in the same system."<sup>84</sup>

In reviews of Pred's Behavior and Location,<sup>85</sup> both Adams<sup>86</sup> and Harvey<sup>87</sup> were critical on this point of the two postulates advanced by Pred as providing the "Foundations for a Geographic and Dynamic Location Theory." Pred had postulated that deviation from behavior expected from models of classical location theory could be explained in terms of inter-individual differences in the amount of information available to a decision-maker and in his ability to use this information. However, the reviews point out that the two postulates are not independent; that,<sup>88</sup> "the ability to use information is a function of its quantity and quality and vice-versa."

#### Measurement Problems in Constructing Behavioral Postulates

A new type of quantification problem follows the decision to construct location theory on a sounder behavioral foundation. Interest in behavioral propositions leads to the formulation of new concepts and to efforts to define these concepts in an unambiguous way. Harvey, by reference to the treatment of "satisficing behavior" in recent geographical work, has illustrated the confusion that results when behavioral concepts are not rigorously defined.<sup>89</sup> Fundamental concepts, requiring the development of sophisticated measurement approaches include: mental images of areas and of attributes of areas; subjective preference structures for areas and for attributes of areas; learning about areas and the formation of pre-conceptions about areas; choice strategies among perceived alternatives; treatment of environmental uncertainty; formation of group attitudes and reconciliation of inter-group differences with respect to spatial problems. For each of these

problem areas, the task of developing adequate measurement techniques has barely begun. The most generally useful approach may well prove to be in the area of multi-dimensional scaling with its versatility in terms of data input forms and basic characteristics and with the recent developments in the scaling of inter-individual differences.

The section below reviews the approach of multi-dimensional scaling and discusses some examples where previously intractable measurement problems have been resolved.<sup>90</sup> Although a review of applications in marketing now exists,<sup>91</sup> very little discussion of scaling has yet appeared in the geographical literature.

### Multi-dimensional Scaling

#### Scaling

The fundamental idea of scaling is to produce a range of scores that have meaning either with respect to each other's values or to some arbitrary or absolute value, set or accepted by the scale. A scale generally consists of a system of numbers related by correspondence rules which enable meaning to be attached to the objects possessing them. For example a number system is a scale which can be normal, ordinal, interval or ratio in nature.<sup>92</sup> The explication of any number system involves itemizing the correspondence rules which give meaning to each number in the system.<sup>93</sup> Generally, the scaling problem reduces to one of devising rules for the measurement of a construct or phenomenon such that the resulting measurements provide an easily interpretable and admissible transformation into numerical form of the



to rate objects according to the magnitude of some specific attribute. Specific examples of these procedures are discussed later.

Recognition that any object may have a number of attributes, and that different attributes may be used by different individuals in their attempts to scale the objects in some way, led to the conclusion that any given object could be regarded as existing in an n-dimensional space, where n represents the number of perceived or actual attributes. The quantity of each attribute belonging to an object can then be interpreted as a geometrical coordinate which, when used in conjunction with other quantities (coordinates), allows one to pinpoint the location of each object in the n-dimensional space.

There are, however, some very fundamental and important points to consider about this geometrical interpretation of the multi-dimensional scaling process. First let us consider the case where objects are located in an n-dimensional real Euclidean space. Here each number associated with an attribute would give the projection of the object on one of the coordinate axes of the space; in other words it would allow us to determine the distance of the object from an origin along a given axis. Using basic geometrical principles, distance between points located in the spaces can consequently be determined as follows:

$$d_{jk} = \left[ \sum_{r=1}^n (p_{rj} - p_{rk})^2 \right]^{\frac{1}{2}}$$

where j, k are any two points in the space  
r is an index of the axes  
n is the number of orthogonal axes  
 $p_{rj}$  is the projection of point j on axis r

What is even more important however, is that given the distance between all pairs of points in the space, the projections of the points on an arbitrary set of orthogonal axes of the space, can be determined. In other words, given interpoint distances, we can recover the number of dimensions in which the points exist. For any set of interpoint distances there will be a space of minimum dimensionality in which a satisfactorily large number of the interpoint distances hold their relationships. One of the aims of the multi-dimensional scaling is to identify this space of minimum dimensionality and thus to allow some interpretation of each dimension in terms of stimulus attributes.

The second critical feature of multi-dimensional scaling is that it is not necessary to have, as input, metric information on the interpoint distances. Because subjects are not asked to make decisions with respect to a given number of attributes, they are free to choose any number of attributes they desire in order to make a distinction between objects. Thus, instead of imagining that each stimulus object has a location in a real Euclidean space, the subject locates each object in a "psychological space". In addition, subjects locate points in the space merely in terms of being "nearer," "greater than," "more similar," "more preferred," (and so on) to any given object than it is to others. The aim of multi-dimensional scaling is to take the data collected with respect to stimulus objects and to recover from it a spatial configuration of points in an identifiable space of minimum dimensionality.

### Data Requirements and Collection

Coombs<sup>97</sup> argues that there are four basic kinds of behavioral data; preferential choice, single stimulus, stimulus-comparison, and similarities data.

Assume that we have a sample of individuals and a collection of phenomena and that the individuals are asked to state their preferences for the phenomena. The instructions may be of the following types:

- a. "Choose one out of a set of n phenomena."
- b. "Choose k out of a set of n phenomena."
- c. "Choose one of the a series of subsets of the n phenomena."
- d. "Choose one out of every possible pair of the phenomena."

When sample members perform one or another of the above tasks they state their preferences for the phenomena chosen and the type of data collected is called preferential choice data. With such data we regard individuals and stimuli as points in a psychological space. A preferential choice of an individual between two stimuli is interpreted to mean that one stimulus point is nearer the individual's ideal point than the other stimulus point.

The second type of data that Coombs defines is single stimulus data. Here we assume that our sample individuals are presented with a set of homogeneous stimuli (i.e. stimuli from a single population such as political candidates, supermarkets, etc.), but this time the individual is asked to make a judgment about each stimulus in turn.

A third type of data arises when we ask individuals to determine an order relation on pairs of points from the same data set: this is called

stimulus comparison data and the method of collecting it is generally known as the method of paired comparisons.<sup>98</sup> When the stimulus are not explicitly identified, but are combined in multi-attribute objects, comparisons of objects yield the fourth type of data-similarities data.

Of the various types of data available, geographers have experimented to some extent with each type. The use of paired comparison procedures for collecting data is becoming somewhat more popular. Examples of such experiments include asking consumers to select one of a pair of shopping centers, brands of goods, towns to visit in order to purchase a given good, or towns which would be selected for purposes of migration. The frequency with which any given pair member is chosen over others is then recorded.<sup>99, 100</sup> For example, Rushton interpreted the movement of farmers to towns in Iowa as the outcome of a choice process which could be inferred to be a paired comparison type procedure. Visits to each place were transformed into dissimilarities measures first by recording the number of times town type  $i$  was chosen (for a specified shopping trip) over town type  $j$  when both  $i$  and  $j$  were present in a feasible area then, regarding a proportion of 0.5 as being maximum perceived similarity, finding the difference between the derived proportions and maximum perceived similarity:

$$\text{i.e. } d_{ij} = \left| P_{ipj} - 0.5 \right|$$

where  $d_{ij}$  is a measure of dissimilarity. Here a small value for  $d_{ij}$  represented small dissimilarity between towns, and a large value indicated considerable dissimilarity. Town types referred to towns in combinations of size classes and distance classes from the farmers.

### Approaches to Non-metric Multi-dimensional Scaling

It was previously hypothesized that, implicit in every collection of proximity measures (such as measures of similarity and dissimilarity) is a kind of spatial structure. The basic problem of MDS is to uncover this structure. While it is generally agreed that greater degrees of similarity infer closer distances (and vice versa), the former are only implied distances and may not easily be transformed into metric form. However Shepard has argued that:

"If some monotonic transformation of the proximity measures could be found that would convert these implicit distances into explicit distances, then we should be in a position to recover the spatial structure contained only latently in the original data."<sup>101</sup>

The various approaches developed by authors such as Coombs, Torgerson, Shepard, Kruskal, McGee, Guttman-Lingoes, and Young represent attempts to recover the latent spatial structures contained in proximity-type data.<sup>102</sup>

The relative advantages of the non-metric approach in searching for latent spatial structure have been summarized by Lingoes and Guttman:

"One of the chief benefits to be derived from constraining the solution non-metrically, is, of course, that in general a smaller space is required to reflect order than to reflect metric. Of greater importance, however, the dimensions themselves may well aid our understanding of the underlying interdependencies free of the attenuation that can result from non-linear relationships. Furthermore, when some lawful structure or pattern is present in the data, e.g., a simplex, a circumplex, or a radex, a non-metric analysis will reveal the configuration whereas a metric approach will obscure the lawfulness."<sup>103</sup>

## The Basic Elements of Non-metric MDS Algorithms

Although there are differences in the variety of non-metric MDS algorithms currently in use, there are also broad similarities in terms of their construction. Features common to the majority of the techniques include:

- a) an initial set of input data, frequently generated by a paired comparisons experiment, within which is contained a latent spatial structure;
- b) an initial configuration of interpoint distances which is manipulated on successive iterations in an attempt to define a monotone relationship between distances measured in the configuration and the corresponding distances in the original data;
- c) a computing algorithm (a non-metric scaling method) which incorporates the strategy for achieving convergence of the data and the configuration;
- d) a loss function (or "goodness-of-fit") function which is used to guide and/or terminate the iterative procedures;
- e) subroutines for handling missing data and tied data, and for determining step size motions within each generated configuration;
- f) techniques for estimating the configuration deformation as the number of dimensions in which the configuration is plotted are changed.

A generalized format for a non-metric MDS analysis of complete data has been provided by Lingoes and Roskam.<sup>104</sup>

### Joint-Space Scaling Solutions

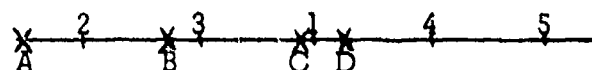
The same principles we have already described can be applied to the problem of locating  $m + n$  points in a space such that the order of the distances between the two sets of points corresponds with the original ordered data.

A simple hypothetical example will illustrate the model. Suppose the ranking of five locational stimuli by four individuals is as shown below:

#### Hypothetical Ranking of Stimuli by Four Sample Groups

Stimuli Locational Types	Groups			
	A	B	C	D
1	3	3	1	1
2	1	2	4	5
3	2	1	2	3
4	4	4	3	2
5	5	5	5	4

Geometrical Model:



Legend:

1 Stimuli  
X Groups

The distances from each of the groups to each of the five stimuli, when rank ordered for each group, have the same order as in the Table shown above, ( $n + m$  coordinates are sufficient to recover  $n \times m$  original data values). The model also allows generalizations to be made about similarity between groups as they order the stimuli. For example, distances between the groups in the model space may be used as a summary of similarity of point of view.

### Interpretations of Scale Dimensions

While the use of MDS has considerable potential for geographic use, one of the most fundamental problems is that of interpreting the dimensions in which configurations are mapped. In some cases this particular problem ceases to exist because the researcher is interested only in the position or relation of the configuration points with respect to each other (for example in determining clusters of stimulus points). However, in other cases the scale value derived from projecting any given point on to an appropriate dimension of the space is sought after. Under these circumstances the interpretability problem arises.

The problem of identifying the dimensions of any configuration has been solved in a number of different ways. For many geographical studies some information is known about the location of stimulus points in an objective space prior to the building of a configuration. When this type of information is known constraints can be placed on the number of dimensions in which the output configuration is produced such that one attempts to replicate the spatial structure of the original objective configuration. Thus in Tobler's use of multi-dimensional scaling algorithms as map transformations, we find that he is able to orient configurations in the same way as preselected map projections are oriented and to interpret distances according to distances measured on these projections.<sup>105</sup> Similarly in urban distance perception studies the locations of the phenomena being investigated are known prior to the beginning of the study. Output configurations can therefore be rotated, reflected and translated until the positional relations in the output configuration



have the same directional components as in the original data. In both these cases the problem of identifying the dimensions themselves are trivial ones.

In cases where the configuration of stimulus points is not known, then considerable ingenuity has been used in order to interpret dimensions. For example in one study a large number of personal, social, economic and attitudinal characteristics were collected for each sample respondent, and various types of characteristics were collected for the stimulus points. By investigating the scores of stimulus points and individuals on uni-dimensional scales of each attribute, the author was able to interpret his dimensions by choosing those attributes which appeared to be most highly correlated with the derived scale values. In another study dimensions of the configuration were interpreted in terms of qualities of the stimulus objects that had been derived from independent scaling analysis.<sup>106</sup> Thus when the configuration of stimulus points is unknown it appears that the most appropriate method for identifying dimensions is to compare the scores of each stimulus point on each dimension with some prior selection of attributes of the stimuli. Those attributes having the highest correlation with scale values then lend themselves to use in interpreting dimensions of the configuration.

#### A Worked Example of the Non-Metric Algorithm

Our discussion to this point has centered on the objectives behind multi-dimensional scaling approaches and on the question of the appropriate

criteria for evaluating goodness-of-fit. Several algorithms have been developed to meet the stated objectives and only recently have any systematic attempts been made to compare the relative performances of the different algorithms.<sup>107</sup> In the following discussion we will explain multi-dimensional scaling through the mechanism of a hand-worked example. In so doing we will apply one of these several scaling algorithms.

The algorithm chosen is that developed by Young and Torgerson<sup>108</sup> and it is applied below to a very simple, hypothetical problem. Later we will discuss realistic examples.

In this example, the similarities between points with which the scaling begins are derived from field questionnaire responses. The existence of a configuration is thus a hypothesis and its true dimensionality is also unknown at the outset. The similarity data with which the example begins is the result of a paired-comparisons treatment of the towns chosen by a random sample of Iowa rural households for major grocery expenditures.<sup>109</sup> Specifically, the data indicates the absolute value of the difference from 0.5 of the relative frequency with which towns at one range of distance and belonging to a town-size-distance classes are regarded as stimuli, and since the stimuli can be decomposed into the two component parts of town size and distance, the purpose of the scaling approach is to determine the relative tradeoff between the two components. Thus, we wish to answer the question for any and all comparisons: by how nearer or farther should a town of a given size be in order to be just preferable to a second town of a given size at a given distance.

The multi-dimensional scaling problem is essentially one of finding the locations of points in a space of any given number of dimensions such that an ordering of the distances between points in this space best corresponds to an ordering of the points in the original input data. Thus, it is implied that the researcher possesses knowledge on the order relations between a set of points (usually from experimental data) and that he hypothesizes that these order relations are derived from a mental configuration of the points (unknown, of course, to the researcher). The purpose of the scaling, then, is to construct a configuration of points from which measurements can be made between points on which the order relation corresponds to the order relation of the interpoint distances in the experimental data.

#### Overall Strategy

The problem is solved in a series of iterations, each one of which comprises four stages. We begin with a random configuration of the six points in two dimensions (the initial configuration) see Table 8 and the strategy for solution is to move successively closer to the solution on each iteration, stopping when the index of fit shows that the previous iteration has resulted in a new configuration that is not superior to that of the previous one.

#### The Four Stages

Step 1. Computation of distances: Distances between the points of any configuration are computed from the formula

$$d_{ij} = \left[ \sum_{k=1}^r (x_{ik} - x_{jk})^m \right]^{1/m} \quad \text{----- (1)}$$

The original similarities are shown in Table 8.

Table 8

ORIGINAL SIMILARITIES

1	0.0	0.430	0.330	0.140	0.400	0.340
2	0.430	0.0	0.500	0.410	0.500	0.470
3	0.330	0.500	0.0	0.370	0.300	0.120
4	0.140	0.410	0.370	0.0	0.500	0.390
5	0.400	0.500	0.300	0.500	0.0	0.500
6	0.340	0.470	0.120	0.390	0.500	0.0

Computing the scale in one dimension the initial (random) configuration had the coordinates:

Point	Random Coordinates
1	0.626
2	0.940
3	0.493
4	0.713
5	0.497
6	0.907

where  $i$  and  $j$  represent any two points in the configuration:  $x_{ia}$  and  $x_{ja}$  represent the projection (coordinate) of points  $i$  and  $j$  respectively on axis  $a$ ;  $r$  is the number of axes (dimensions) for which the solution is being computed; and  $m$  is the minkowski constant determining the type of distance metric for which solution is being determined: e.g., euclidean distance has the minkowski constant of 2.<sup>110</sup>

Step II. Computation of monotonically transformed distances (disparities):

The purpose of this step is to constrain the distances from the given configuration so that they do not violate the order relation of the original interpoint distances (in this case, Table 8). This is achieved by ensuring that if one were to plot the values of this table (y axis) against the monotonically transformed values of Table 9 (x axis), the "curve" joining these points would never move to the left, but only vertically or to the right. These monotonically transformed distances are known as "disparities"; they are not distances from any known configuration but rather, are a monotone sequence of numbers as "nearly equal" to the distances in the given configuration as is possible without violating the original order relation of interpoint distances. The purpose of non-metric multi-dimensional scaling is, of course, to construct a configuration of points in any given number of dimensions such that the interpoint distances and the monotonically constrained distances (disparities) are as similar as is possible.<sup>111</sup>

The disparities are computed by taking the interpoint distance from the configuration (Table 9) corresponding to the two most similar points (smallest distance) in the original data (Table 8) and comparing it with the

The distances and disparities for this initial configuration are shown below:

Table 9

Initial Distances

	1	2	3	4	5	6
1		.314	.133	.087	.129	.281
2			.447	.227	.443	.033
3				.220	.004	.414
4					.216	.194
5						.410
6						

Initial Disparities

	1	2	3	4	5	6
1		.199	.160	.160	.199	.199
2			.447	.199	.443	.199
3				.199	.160	.160
4					.216	.199
5						.410
6						

distance that corresponds with the next two most similar points. If the first distance is smaller than the second, then the order of the distances corresponds with the order of the original similarities and no transformation is necessary. However, if the first distance is larger than the second, then the original order relation of interpoint distances is violated, and since the disparities must not decrease in value when the similarities increase, the arithmetic mean of the two distances is substituted for the distances and this mean becomes the first two disparity values. The interpoint distance from the configuration corresponding to the third smallest original similarity (Table 8) is then compared with the second disparity (which might be the second distance or the mean of the first two distances -- as discussed above). If the third distance is larger than the second disparity, it becomes the third disparity; otherwise the mean of it and the previous disparity (weighted mean if the previous disparity was composed of more than one distance) is computed, and this mean becomes the disparity value for the third distance and second distance (also for the first distance if the second disparity was itself a mean value). In the first iteration of the problem described below, the disparities will be computed step by step following the procedure outlined above.

Step III. Computation of goodness-of-fit (stress): The measure of goodness-of-fit is a measure of how far the disparities ( $\hat{d}_{ij}$ ) depart from the distances measured from the derived configuration ( $d_{ij}$ ). The larger these departures are, as compared with the distances themselves, the greater the error in reproducing the order relation of the original similarities from the derived

configuration, and therefore, the poorer is the fit. One frequently used measure of fit is Kruskal's "stress" value, defined as:<sup>112</sup>

$$\text{stress} = S = \sqrt{\frac{\sum_{i < j} (d_{ij} - \hat{d}_{ij})^2}{\sum_{i < j} d_{ij}^2}} \quad \text{-----} \quad (2a)$$

Roskam points out that, while formula (2a) is independent of the scale of  $d$ , it is not independent of the scale of  $\hat{d}$ <sup>113</sup> and that in cases where  $d$  would be an interval scale, the more appropriate stress formula is:

$$\text{stress}_2 = S_2 = \sqrt{\frac{\sum_{i < j} (d_{ij} - \hat{d}_{ij})^2}{\sum_{i < j} (d_{ij} - \bar{d})^2}} \quad \text{-----} \quad (2b)$$

Where  $\bar{d}$  is the mean of the  $d_{ij}$ .

The smaller the stress, the better the fit. In the example developed below, stress will be computed from both formulae.

Step IV. Computation of a new (improved) configuration: The greater the discrepancy between the distances and disparities from any configuration, the poorer the configuration. Therefore, to improve any given configuration, each point should be moved so as to reduce the average discrepancy between the distances and the disparities with respect to the other points.<sup>114</sup> If  $d_{ij} > \hat{d}_{ij}$  then point  $i$  should be moved closer to point  $j$  by an amount proportional to the size of the discrepancy. Thus, ceteris paribus, after this adjustment, the discrepancy  $d_{ij} - \hat{d}_{ij}$  should be smaller on the new configuration than on the previous one. However, since for each of  $n$  points there are



$n - 1$  distances to the other points, there will be  $n - 1$  possible adjustments for each point. The mean of these possible adjustments is the actual adjustment that is made. In the worked example that follows, the formulae used are from Young.<sup>115</sup> The displacement of point  $i$  with respect to point  $j$  is given by:

$$c_{ija} = \alpha (d_{ij} - \hat{d}_{ij}) (x_{ja} - x_{ia}) / d_{ij} \quad \text{-----} \quad (3a)$$

Where  $d_{ij}$  are distances computed from the previous configuration  
 $\hat{d}_{ij}$  are the disparities (monotonically transformed distances) from  
the previous configuration

$x_{ja}$  and  $x_{ia}$  are the coordinates of points  $j$  and  $i$  respectively on  
axis  $a$

$\alpha$  is a constant of proportionality.

The new position of point  $i$  on axis  $a$  is the coordinate on the previous configuration plus the means of the correction vectors defined in (3a) above:

$$c_{ia} = \frac{\alpha}{n} \sum_{j=1}^n (d_{ij} - \hat{d}_{ij}) (x_{ja} - x_{ia}) / d_{ij} \quad \text{-----} \quad (3b)$$

$$x_{ia}^1 = x_{ia} + c_{ia} \quad \text{-----} \quad (4)$$

Alternative formulae for reaching the same goal are discussed elsewhere.

In the literature of multi-dimensional scaling, alpha is commonly referred to as the "step-size parameter". Much discussion there has centered on the question of modifying the value of alpha as the iterations converge on a solution.<sup>117</sup> In the method of computing revised configurations used in this example, larger values of alpha would speed up convergence (that is, lead to a given solution in a small number of iterations) subject,

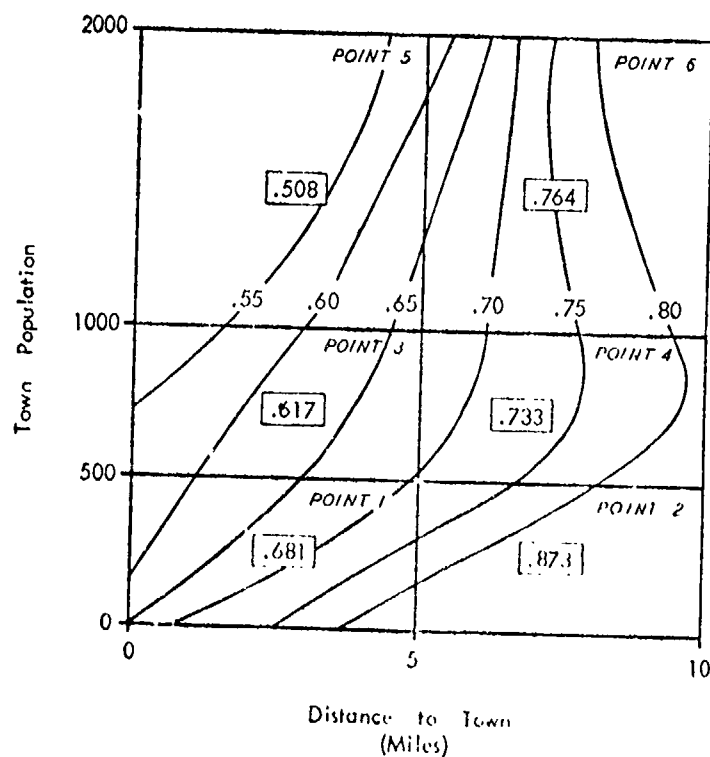
however, to the constraint that if alpha becomes too large, we run into the danger of moving the points in any revised configuration too far. In this example, for the sake of simplicity, alpha has the value, 1.0.

In computing the disparities, a problem resulting from ties in the original similarities is encountered. It is solved here using the "primary approach".<sup>118</sup> In this approach an order relation within a tie is determined from the numerical order of the corresponding distances from the configuration. This is in contrast to the "secondary approach" in which the distances from the configuration are first averaged into a block corresponding to any tied values in the original similarities data. Since the primary approach may result in disparities that are different for tied values in the original data, whereas this cannot happen with the secondary approach, the former is known as the weak monotonicity approach and the latter is known as the semi-weak monotonicity approach.<sup>119</sup>

After five iterations, the value of stress was .230 and no further reduction of this value took place during the next twenty-five iterations that were computed. Substantive interpretation of the resulting configuration is facilitated by Figure 28 where the scale values are isoplethed for graphical display of the tradeoffs between the two conflicting stimuli of distance and town size of consumer movement. From this figure it is apparent that for the purposes of making grocery expenditures towns of less than 1,500 population owe much of the patronage which they receive from the rural population to their proximity. In a rather regular fashion, the Iowa rural population substitutes stores in small towns at close distances for stores in larger towns

FIGURE 28

Scale Values after the Fifth Iteration  
Interpolated Preference Structure



Legend: Point numbers reference the stimuli numbers in the tables in which scale values are computed.

Numbers in boxes are scale values after the 5th iteration.

ITERATION 1

POINT 1 on AXIS 1

$$\begin{array}{ll} 2 & (0.31 - 0.20) \times (0.94 - 0.63)/0.31 = 0.12 \\ 3 & (0.13 - 0.16) \times (0.49 - 0.63)/0.13 = 0.03 \\ 4 & (0.09 - 0.16) \times (0.71 - 0.63)/0.09 = -0.07 \\ 5 & (0.13 - 0.20) \times (0.50 - 0.63)/0.13 = 0.07 \\ 6 & (0.28 - 0.20) \times (0.91 - 0.63)/0.28 = 0.08 \end{array}$$

0.94 is mean move (alpha= 1.00)

POINT 2 on AXIS 1

$$\begin{array}{ll} 1 & (0.31 - 0.20) \times (0.63 - 0.94)/0.31 = -0.12 \\ 3 & (0.45 - 0.45) \times (0.49 - 0.94)/0.45 = 0.0 \\ 4 & (0.23 - 0.20) \times (0.71 - 0.94)/0.23 = -0.03 \\ 5 & (0.44 - 0.44) \times (0.71 - 0.94)/0.44 = 0.0 \\ 6 & (0.03 - 0.20) \times (0.91 - 0.94)/0.03 = 0.17 \end{array}$$

0.00 is mean move (alpha = 1.00)

POINT 3 on AXIS 1

$$\begin{array}{ll} 1 & (0.13 - 0.16) \times (0.63 - 0.49)/0.13 = -0.03 \\ 2 & (0.45 - 0.45) \times (0.71 - 0.49)/0.45 = 0.0 \\ 4 & (0.22 - 0.20) \times (0.91 - 0.49)/0.22 = 0.02 \\ 5 & (0.09 - 0.16) \times (0.50 - 0.49)/0.09 = -0.16 \\ 6 & (0.41 - 0.16) \times (0.91 - 0.49)/0.41 = 0.25 \end{array}$$

0.02 is mean move (alpha = 1.00)

POINT 4 on AXIS 1

- 1  $(0.09 - 0.16) \times (0.63 - 0.71)/0.09 = 0.07$
- 2  $(0.23 - 0.20) \times (0.94 - 0.71)/0.23 = 0.03$
- 3  $(0.22 - 0.20) \times (0.49 - 0.71)/0.22 = -0.02$
- 4  $(0.22 - 0.22) \times (0.71 - 0.71)/0.22 = 0.0$
- 5  $(0.19 - 0.20) \times (0.91 - 0.71)/0.19 = -0.00$

0.01 is mean move (alpha = 1.00)

POINT 5 on AXIS 1

- 1  $(0.13 - 0.20) \times (0.63 - 0.50)/0.13 = -0.07$
- 2  $(0.44 - 0.44) \times (0.94 - 0.50)/0.44 = 0.0$
- 3  $(0.00 - 0.16) \times (0.49 - 0.50)/0.00 = 0.16$
- 4  $(0.22 - 0.22) \times (0.71 - 0.50)/0.22 = 0.0$
- 5  $(0.41 - 0.41) \times (0.91 - 0.50)/0.41 = 0.0$

0.01 is mean move (alpha = 1.00)

POINT 6 on AXIS 1

- 1  $(0.28 - 0.20) \times (0.63 - 0.90)/0.28 = -0.08$
- 2  $(0.03 - 0.20) \times (0.94 - 0.90)/0.03 = -0.17$
- 3  $(0.41 - 0.16) \times (0.49 - 0.90)/0.41 = -0.25$
- 4  $(0.19 - 0.20) \times (0.71 - 0.90)/0.19 = 0.00$
- 5  $(0.41 - 0.41) \times (0.50 - 0.90)/0.41 = 0.0$

I

CONFIGURATION

1

1	0.663
2	0.944
3	0.508
4	0.725
5	0.511
6	0.824

II

DISTANCES

	1	2	3	4	5	6
1	0.0	0.281	0.155	0.063	0.151	0.161
2	0.281	0.0	0.436	0.218	0.432	0.120
3	0.155	0.436	0.0	0.217	0.003	0.316
4	0.063	0.218	0.217	0.0	0.214	0.099
5	0.151	0.432	0.003	0.214	0.0	0.313
6	0.161	0.120	0.316	0.099	0.313	0.0

III

DISPARITIES

	1	2	3	4	5	6
1	0.0	0.026	0.155	0.127	0.157	0.157
2	0.206	0.0	0.349	0.206	0.349	0.206
3	0.155	0.349	0.0	0.157	0.127	0.127
4	0.127	0.206	0.157	0.0	0.349	0.157
5	0.157	0.349	0.127	0.349	0.0	0.349
6	0.157	0.206	0.127	0.157	0.349	0.0

IV

$$\text{STRESS} = \sqrt{\frac{.11}{.89}} = 0.3497$$

ITERATION 2

I

CONFIGURATION

	1
1	0.666
2	0.915
3	0.544
4	0.741
5	0.517
6	0.793

II

DISTANCES

	1	2	3	4	5	6
1	0.0	0.249	0.123	0.075	0.150	0.127
2	0.249	0.0	0.372	0.174	0.399	0.122
3	0.123	0.372	0.0	0.197	0.027	0.250
4	0.075	0.174	0.197	0.0	0.224	0.052
5	0.150	0.399	0.027	0.224	0.0	0.277
6	0.127	0.122	0.250	0.052	0.277	0.0

III

DISPARITIES

	1	2	3	4	5	6
1	0.0	0.186	0.123	0.117	0.150	0.126
2	0.186	0.0	0.318	0.174	0.318	0.186
3	0.123	0.318	0.0	0.126	0.117	0.117
4	0.117	0.174	0.126	0.0	0.318	0.126
5	0.150	0.318	0.117	0.318	0.0	0.318

IV

STRESS = 0.3081



ITERATION 5

STRESS = .230

I

CONFIGURATION

1

1	0.681
2	0.873
3	0.617
4	0.733
5	0.503
6	0.764

II

DISTANCES

	1	2	3	4	5	6
1	0.0	0.192	0.064	0.053	0.172	0.083
2	0.192	0.0	0.256	0.140	0.365	0.109
3	0.064	0.256	0.0	0.117	0.108	0.147
4	0.053	0.140	0.117	0.0	0.225	0.031
5	0.172	0.365	0.108	0.225	0.0	0.256
6	0.083	0.109	0.147	0.031	0.256	0.0

III

DISPARITIES

	1	2	3	4	5	6
1	0.0	0.153	0.086	0.068	0.153	0.086
2	0.153	0.0	0.275	0.153	0.275	0.153
3	0.086	0.275	0.0	0.086	0.086	0.086
4	0.068	0.153	0.086	0.0	0.275	0.086
5	0.153	0.275	0.086	0.275	0.0	0.275

at larger distances. The advantage of the results of the scaling procedure is that, for the first time, we are able to state in an exact fashion the circumstances in which one town will be more favorable than another for any and all rural locations.

There is evidence, however, that local minima problems were encountered in this illustration. The example started with a random (initial) configuration even though, as discussed in the section above, beginning the non-metric algorithm in such a way increases the likelihood of ending in local minima traps. Professor Lingoes from the University of Michigan analyzed this data with MINISSA-1 and obtained a stress of .119 for the six points with coordinates (approximately) of 5, 100, -41, 15, -100 and -19 respectively for the six points.

#### Geographical Application of MDS Analysis

The variety of actual and potential applications of MDS is large and ranges over many subsets of the field of geography. Perhaps the simplest way to arrange examples is to divide them into those that use simple space configuration (i.e. configurations of only the stimulus objects) and joint space configuration (i.e. those which map the configurations of both individuals and stimulus objects).

##### Simple Space Outputs

We have constantly stressed that MDS programs can accept both metric and non-metric input: it seems reasonable therefore to give specific examples of each type of research problem.

### Map Transformations

Probably one of the most quoted examples of metric input to a MDS program with simple space output, is Greenberg's "Roadmap" problem.<sup>120</sup> The basic data here was the intercity road mileages between all pairs of 15 cities in the U.S. thus  $105$  or  $\frac{n(n-1)}{2}$  intercity distances were taken from a road atlas and comprised the metric input data. The data was interpreted as similarities data by ranking distances from 1 - 105 on the basis of magnitude, with the shortest road distance represented by rank 1 and the largest distance by rank 105. The output consisted of a configuration of points in two dimensions (north-south and east-west). Discrepancies between predicted configurations - obtained from a non-metric multi-dimensional scaling program - and actual configurations were for the most part small, and could be accounted for by the simple fact that road distances are frequently not the shortest distances between places, but reflect detours around natural and man-made barriers. In other words, locations outputed by the program represent "true" locations if all road connections between the pairs of cities were straight lines. The solution here represents a type of map transformation similar to that which would be achieved if places were located on an elastic map, joined by lines representing actual roadways, then the elastic was stretched in each direction until all road lines were straightened out.

Note that in this example where metric input is used, the first step in the Kruskal algorithm is to convert the metric data to non-metric (ranked) form. Thus the final configuration of points is obtained from non-metric information. This is true of all analyses in non-metric multi-dimensional

scaling. However, where the researcher can have confidence in the metric information he begins with, it is often advantageous to resort to a metric multi-dimensional scaling. Tobler has pointed out<sup>121</sup> that the problem of constructing map projections is one in which the final metric is usually known to be euclidean and the number of dimensions known to be two.

Accordingly, he argues that empirical map projections might be designed to produce, from a matrix of empirical distances between points, the "best" configuration of the points in two dimensions such that the sum of the squares of the difference between the original distances and the resulting configuration (map) distances are minimized. In other words, instead of beginning with a definition of properties in the abstract that must be preserved, his procedure would begin with observed distances between points in the space and design a projection to best replicate these measurements. After he had computed the 2,080 spherical distances between a set of 65 regularly spaced latitude and longitude intersections covering the United States, Tobler used metric multi-dimensional scaling<sup>122</sup> to derive the plane map projection coordinates for the 65 points. Comparing the distances measured from this recovered configuration with the original spherical distances, he showed that the distortion values were generally less than two percent. These distortion values, he concluded, compare favorably with those on Albers' and Lambert's conical projections with two standard parallels.

In a second example of the use of scaling procedures to construct maps, Tobler, Mielke, and Detwyler scaled the geobotanical distances between New Zealand and some neighboring island using inter-island distances inferred

from a model of the diffusion of plant species.<sup>123</sup> Basically the authors attempt to examine the degree to which floristic similarities between New Zealand and its neighboring islands (of which there are eleven in all) can be explained by two geographic factors; (a) the relative position of the islands and (b) the sizes of the islands. The critical question is, what proportion of the commonality of plant species can be explained fully by these two factors? The model they construct attempts to answer this question by using floristic relations to define geobotanical distances which are then compared statistically with the islands' relative locations on the surface of the earth. In other words they attempt to identify quantitatively the floristic relations of localities. They then use these relations (expressed by the number of species common to pairs of islands) together with island size and assumed interaction between islands, to draw their geobotanical map. The distances between islands on the map are then compared with great circle distances to give some measure of the model's worth. Both the geobotanical distances and the great circle distances are input into a multi-dimensional scaling program (Guttman-Lingoes SSA-1) and output in a 2-dimensional Euclidean space. In this way the authors obtain an empirical map projection optimized for the representation of the data. This projection preserves positional relations in the least squares sense more accurately than any other possible map projection. The actual fit of the maps to the distances is approximately 98%. In specifying the output configuration in these terms the authors simplify the interpretation of the dimensions of each configuration for they represent merely the north, south, east, and west dimension common

to any other map projection.

#### Applications of Scaling Principles in Central Place Theory

The same principle of finding a configuration of points that best represents a set of interpoint distances has been used to solve the problem of predicting the location of central places in an area of non-uniform population density. As was illustrated in the first example in this chapter with the case of agricultural location patterns in areas of variable environmental characteristics, adapting classical location theory to fit such cases is only possible by resorting to computer algorithms. Such is the case with this example.<sup>124</sup>

A system of central places can be construed as a pattern of points from each of which a group of entrepreneurs serve a surrounding hinterland. There are expected to be uniform in population size for any class of center. Where environmental conditions are uniform, the point patterns of producers are typically uniform. However, where relevant environmental conditions are spatially variable, corresponding point patterns become distorted. In the case of central place patterns, empirical proof that such distortions occur were provided by King<sup>125</sup> who showed that patterns of central places were most uniform in those areas of the United States where manufacturing activities were largely absent and where population densities were most uniform. More sensitive tests by King<sup>126</sup> confirmed the earlier study by showing that, among randomly selected towns in the U.S., the distance separating a town from its nearest neighbor of the same size or larger was inversely related to local population density. Though these results are

consistent with central place theory, no satisfactory re-formulation of the theory had been made to take account of the non-uniform environment in a formal sense. Rushton proposed a computerized iterative algorithm to solve this program.<sup>127</sup> The algorithm makes successive adjustments to an initial (uniform) configuration of points until it derives the best two-dimensional configuration that satisfies a set of computed expected interpoint distances. In this respect the algorithm belongs to the general class of multi-dimensional scaling algorithms. It may be regarded as a transformation which maps each trial point into a new trial point.

The expected distances are estimates of what the distances between neighboring points would be if the local population density prevailed throughout the area, the points were located in a triangular lattice, and the natural tributary areas around each point contained populations that were equal to threshold demand. After the configuration of points has been adjusted to reflect these distances (by the convergence process described below), revised estimates of the interpoint distances are computed. It is necessary to compute revised distance estimates as the iterations proceed since the points move across the density of demand surface during the iterations. (In this respect the algorithm appears to be quite different from other multi-dimensional scaling approaches.) Since the goodness-of-fit measures that are used rely on the degree to which the distances in the resulting configuration of points correspond with the estimated interpoint distances, were these not representative of the local population densities where the points are located at the close of the iterations, such measures could not be used to evaluate the

performance of the algorithm. Iteration proceeds until the distribution of points is most optimal with respect to the non-uniform population distribution. Criteria for ensuring that the optimal pattern is reached are discussed below.

#### Estimating the Number of Service Points

The first step in the algorithm is to estimate the number of points that will be needed to service the study area. It is assumed that a population density function over the space of the study area has been defined and that a given number of people (threshold) are to be serviced from each point. In this step the total population of the study area is estimated by computing population density for small units of area and thus the population of those areas and summing over the study area. Dividing this total by the threshold size gives the number of points to be distributed. This number of points is then located in a uniform pattern according to a triangular lattice in which all interpoint distances are equal (Fig. 29).

#### Deriving the Estimated Interpoint Distances

The object of the estimated interpoint distances is to express the expected distances between neighboring points that would leave the points with a tributary area surrounding them with purchasing power approximately equal to threshold demand. These distances can be derived from some of the elementary properties of hexagonal lattices. These have been reviewed by Dacey<sup>128</sup> who has also stated the relationship between distances between adjacent centers and the area of the dirichlet regions in such lattices.<sup>129</sup> He states that:<sup>130</sup>



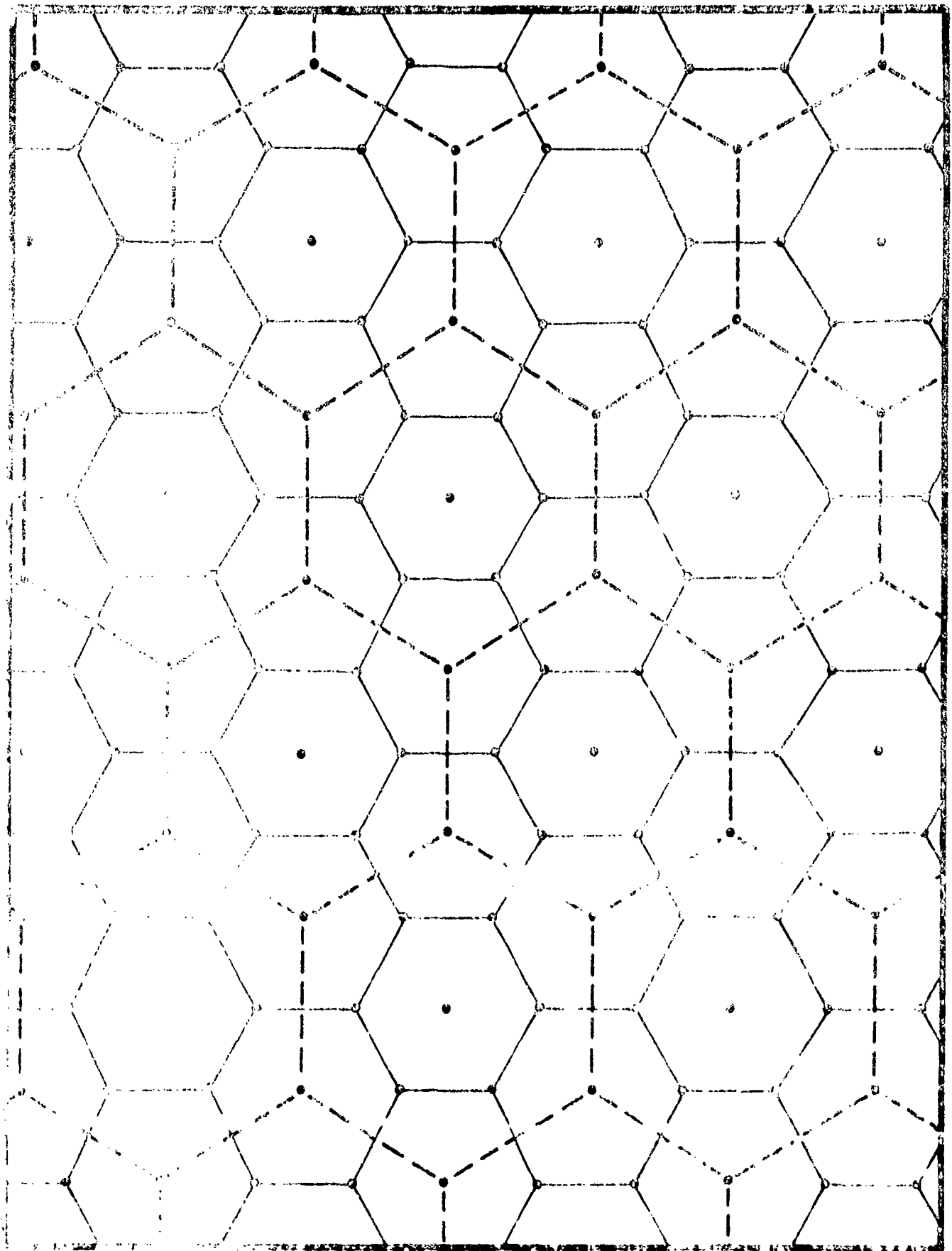


FIG. 21 The Triangular Point Lattice for First Iteration

On the hexagonal lattice the area,  $H$ , of Dirichlet regions with distance  $t$  between centers of adjacent regions is  $H = t^2(3/2)^{1/2}$ . ----- (1)

If, for any location, the population density is known, then the area of the tributary area needed to supply threshold can be computed (assuming that the population density is constant over the local area surrounding the location in question). The area required to support the  $m$ th good is:

$$H = T_m / ((p_i + p_j)/2) \quad \text{----- (2)}$$

where  $p_i$  and  $p_j$  are respectively the population densities for locations  $i$  and  $j$  for which an estimate of expected interpoint distance is required. Woldenberg has pointed out that formula (1) above is incorrect and should be:<sup>131</sup>

$$H = \sqrt{3/2} t^2 \quad \text{----- (3)}$$

and thus

$$t = \sqrt{H \cdot 2 / \sqrt{3}} \quad \text{----- (4)}$$

The estimated distances ( $d_{ij}^*$ ) between market centers in the area for which average population density is  $(P_i + P_j)/2$  is thus:

$$d_{ij}^* = \sqrt{T_m / ((p_i + p_j)/2) \cdot 2 / \sqrt{3}} \quad \text{----- (5)}$$

For a plain non-uniform population density, the lattice of the configuration of points being constructed is, of course, not hexagonal and therefore equation (5) does not apply. However, the intent is to construct a configuration of points that is as close to a hexagonal lattice as possible in the local area--even though over the larger region distortions of the lattice may be pronounced. For this reason equation (5) gives the most suitable approximation to the required distances.

### Calculating Local Population Density ( $p_j$ )

Assume that a continuous function exists for population density over area  $g(x,y)$ . For any set of locations in this area, tributary areas may be defined by thielson polygons (the classical distance-minimizing postulate), by generalized distance-decay functions; or by space preference functions. For all tributary areas so defined, both areas and population may be estimated. Thus  $p_i$  and  $p_j$  may be computed for all places. From the computed values of interpoint distance ( $d_{ij}^*$ ) the locational coordinates of the towns are derived from the method described below.

### Deriving the New Configuration

The role of the  $d_{ij}^*$  is that they are target distances to which a two dimensional set of points should be fitted. Of the several methods available, the one proposed is adapted from the literature of multi-dimensional scaling.<sup>132</sup> In this discussion the  $d_{ij}^*$  distances are referred to as the estimated interpoint distances and the distances computed from the location coordinates of the towns are referred to as the configuration distances ( $d_{ij}$ ).

At each iteration each point is moved to a new position. The amount and the direction of the move is made to depend upon the differences between the configuration interpoint distances and the estimated interpoint distances ( $d_{ij}^*$ ) that take account of the variable population density surface.

Configuration distances are:

$$d_{ij} = \sqrt{\sum_{a=1}^2 (x_{ia} - x_{ja})^2} \quad \text{----- (6)}$$

where  $x_{ia}$  is the projection of the  $i$ th point on axis  $a$ .

For example, if  $d_{ij}$  is larger than  $d_{ij}^*$  then point  $i$  is moved toward point  $j$  by an amount proportional to the size of the difference--and vice-versa.

Each point will have associated with it six correction vectors, one for each of the six neighbors. The mean correction vector for any point on any axis is then added to that point on that axis. When this process is completed for all points on all axes a new configuration exists.

The correction vector is defined as:

$$c_{ija} = \alpha (d_{ij} - d_{ij}^*) (x_{ja} - x_{ia}) / d_{ij} \quad \text{----- (7)}$$

where  $\alpha$  is a constant of proportionality, and  $x_{ia}$  is the projection of point  $i$  on axis  $a$ .

The mean correction vector is thus:

$$c_{ia} = \frac{\alpha}{k} \sum_{j=1}^k (d_{ij} - d_{ij}^*) (x_{ja} - x_{ia}) / d_{ij} \quad \text{----- (8)}$$

where the  $j$  subscripts refer to each of the  $k$  near neighbors of point  $i$ .

The new position of point  $i$  on axis  $a$  is given by:

$$x_{ia}^1 = x_{ia} + c_{ia} \quad \text{----- (9)}$$

The projections  $x_{ia}^1$  describe a new configuration, one that should fit more closely the estimated interpoint distances that show the configuration's adjustment to the non-uniform density surface.

#### Goodness-of-Fit Criteria

One index of goodness-of-fit is the extent to which the distances in the new configuration compare with the estimated distances. An index ( $S_d$ ) can be constructed that will systematically decrease with each iteration if the configuration distances fit more closely to the estimated distances.

$$S_d = \sqrt{\frac{\sum_{j=1}^n \sum_{i=1}^k (d_{ij} - d_{ij}^*)^2}{\sum_{j=1}^n \sum_{i=1}^k d_{ij}^2}} \quad \text{-----(10)}$$

where the  $j$  index refers to all  $n$  points in the study area and the  $i$  index refers to the  $k$  near neighbors of the  $j$ th point.

While  $S_d$  is a useful index for controlling the iterative process, a more appropriate final goodness-of-fit criteria is the degree to which the populations of the tributary areas differ from the specified population norm ( $\text{Raw}_j D_p$ ).

$$\text{Raw}_j D_p = \sqrt{\frac{\sum_{i=1}^n (P_{ji} - T_m)^2}{n}} \quad \text{----- (11)}$$

where  $P_{ji}$  is the population of the tributary area of the  $i$ th point after the  $j$ th iteration;  $T_m$  is the threshold population norm for the  $m$ th good and  $n$  is the number of tributary areas completely contained in the study area.

In so far as the tributary areas surrounding points on a final configuration developed in a heterogeneous area deviate in shape from the hexagonal figure, the estimated distances between points will be systematically underestimated since their estimation assumed a perfect triangular lattice. When the lattice is distorted interpoint distances have to be somewhat larger than expected from theory in order for the tributary areas to realize threshold population. This systematic bias will yield the relationship (that can be empirically confirmed):

$$\frac{\sum_{i=1}^n P_{ji}}{n} = T_m \quad \text{-----(12)}$$

In view of this bias, the variability of the population sizes of the tributary areas may be measured directly as:

$$\text{St. Dev. } {}_jD_p = \sqrt{\frac{\sum_{i=1}^n ({}_jP_i - {}_j\bar{P})^2}{n}} \quad \text{----- (13)}$$

Only when the final tributary areas are compact in shape will  $\text{Raw } {}_jD_p \triangleq \text{St. Dev. } {}_jD_p$ . Always  $\text{Raw } {}_jD_p \geq \text{St. Dev. } {}_jD_p$ . In the classic central place solution (on a uniform plain), both  $\text{Raw } {}_jD_p$  and  $S_d$  will equal zero.

Though the variability of tributary populations after the final iteration is a measure of goodness-of-fit, it is nevertheless a raw measure of fit since it is unrelated to the initial population variability for the uniform distribution of points. Traditional goodness-of-fit criteria requires a statement of the degree to which the initial variance is reduced by the employment of the model. Thus the square root of the ratio of the remaining variance to original variance may be computed and called normalized  $D_p$ .

$$\text{Normalized } {}_jD_p = 1 - \sqrt{\frac{\sum_{i=1}^n ({}_jP_i - T_m)^2}{\sum_{i=1}^n ({}_oP_i - T_m)^2}} \quad \text{----- (14)}$$

In addition to the desire to minimize the variability of the tributary population we also wish to achieve maximum packing density of the primitive Dirichlet regions. Dacey has defined density of packing as:<sup>133</sup> "the ratio of the area of a circle to the area of the polygon in which the circle is inscribed....the tessellation of regular hexagons has the maximum packing density, the density being  $\pi/\sqrt{12} = .9069$ .... Thue (1892)."

### Computerized Version of the Algorithm

The five steps described above were programmed for computer solution. Tests were made for four trend surface functions of increasing degrees of complexity. The results are summarized in Table 10 and in Figs. 29-38. On all trials the change in stress value (Fig. 38) shows a systematic decline indicating smooth convergence in the adjustments to the initial configuration in the direction of the final goal. The results show that the more spatially complex the trend surface functions and the greater the variability of population density in the area, the higher the stress values after a comparable number of iterations. Figure 34, for example, shows the distribution of population density peaking in two areas of the map from a high of ten persons per unit area to a low of 1.5. The high stress values shown in Fig. 38 (third trial) illustrates the difficulty of solving the problem for such a complex density surface. In all cases the population sizes of the tributary areas are close to the respective threshold values though it was noted that the more distorted the tributary areas (see the measure of packing density) the smaller the population of the area relative to threshold size.

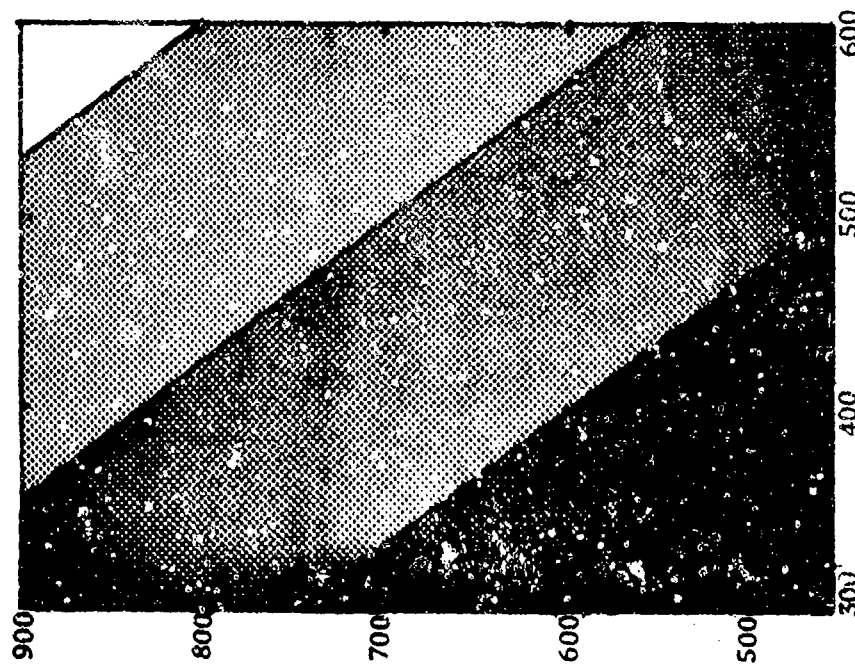
As is common with heuristic algorithms such as this, formal proof of convergence in all circumstances cannot be provided. Strategies for speeding the convergence process by manipulating the value of alpha (equation (7)) as the iterations proceed are being investigated.

This example has illustrated how several independently developed research trends were focused on a problem that has been of long-standing interest in geography. Many such problems will probably yield to similar

TABLE 10: SUMMARY RESULTS OF TESTS OF THE MAP TRANSFORMATION ALGORITHM

Trial Number	1		2	3	4
Function Tested	$z=a+b_1x+b_2y$		$z=\frac{a}{1+\sqrt{x^2+y^2}}$	$z=\max\left[\left(\frac{a}{1+x^2+y^2}\right), \left(\frac{a}{1+(x+b)^2+(y+c)^2}\right)\right]$	$z=a+b_1 \sin x +b_2 \sin y $
1. z=Pop. density x,y.Cartesian Coords.	shed roof		peak cone	double-cone	sin wave
2. Number of pts.	204		247	442	384
3. Number of iterations	15	65	45	47	40
4. Initial stress value	.2679	.2679	.1660	.3723	.2082
5. Final stress value	.1126	.0357	.0532	.1651	.0575
Level One Centers:					
6. Target Pop.	1800	1800	3000	2000	4500
7. Mean Area Pop.	1641	1793	2970	1818	4346
8. Raw Discord (Raw D) j p	220.4	65.3	186.8	435.67	322.0
9. St. Dev. Area Pop.	153.2	65.1	184.9	396.3	283.4
10. Normalized Discord	.5985	.8811	.7530	.5181	.7299
11. Mean Packing Density	.7129	.7629	.7236	.6393	.7342
Level Two Centers:					
12. Target Pop.	5400	5400	9000	6000	13500
13. Mean Area Pop.	4902	5382	8938	5453	13034
14. Raw Discord	686.7	133.6	538.7	1309.3	984.7
15. St. Dev. Area Pop.	472.5	132.9	535.3	1189.9	868.9
16. Normalized Discord	.5960	.9214	.7587	.5158	.7280
17. Mean Packing Density	.7077	.7568	.7211	.6404	.7312





$Z = 4.5 - .003X - .002Y$

Fig.30 Population Density Function, for First Trial

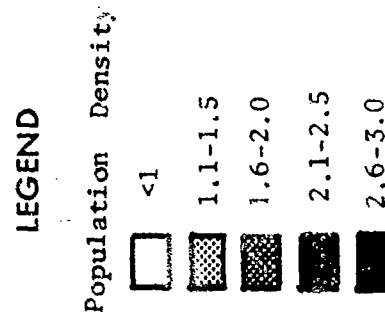
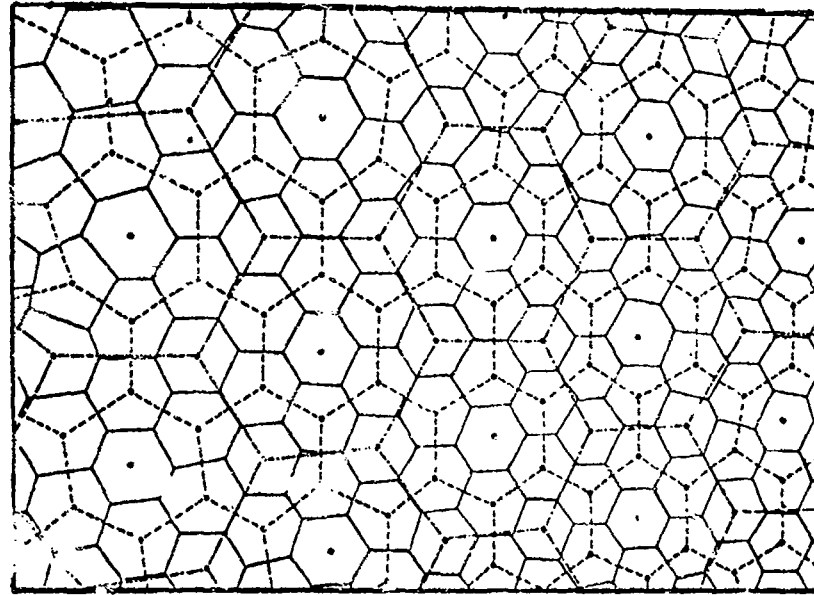


Fig.31 Distribution of Points after 45 Iterations  
for First Trial

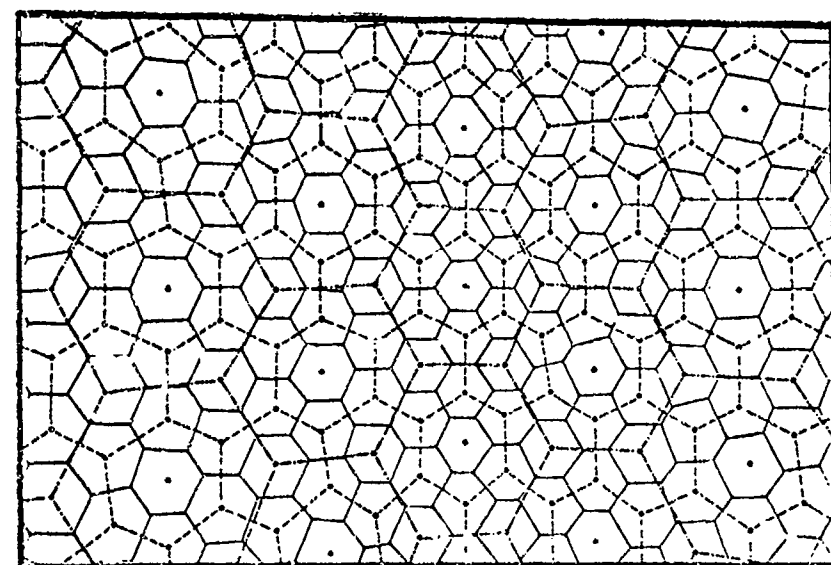


Fig. 33 Distribution of Points after 70 iterations  
for Second Trial

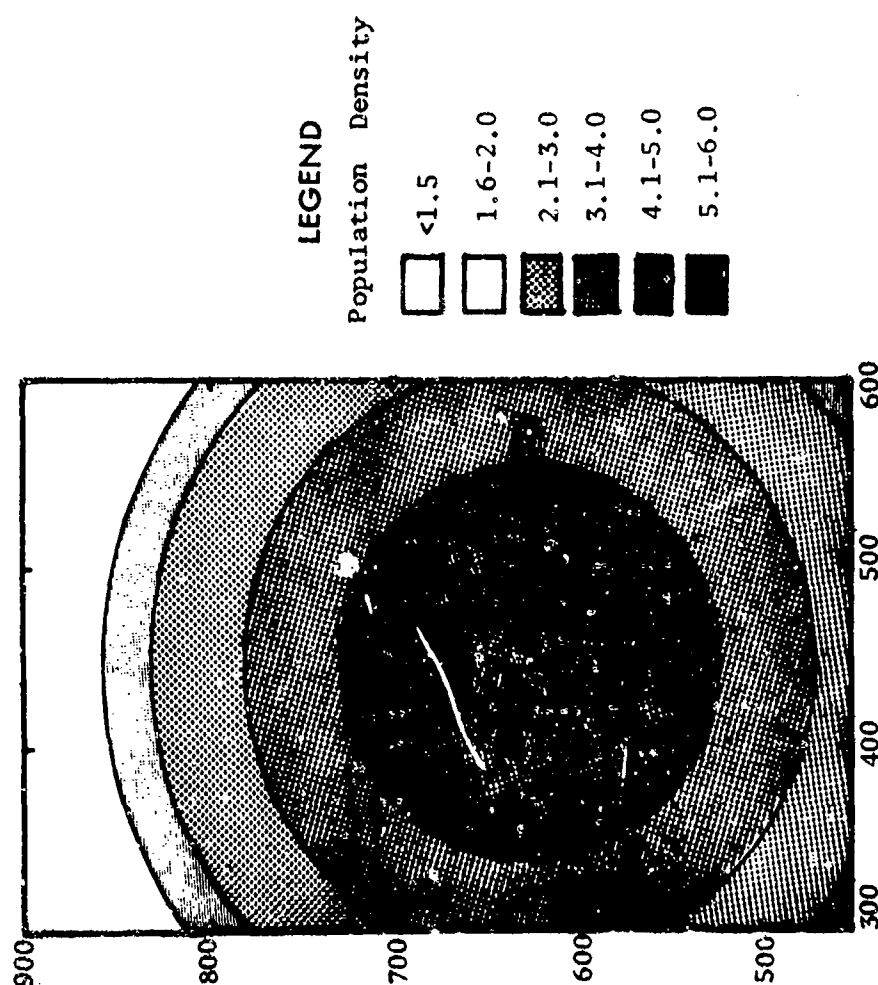


Fig. 32 Population Density Function for Second Trial

$$Z = 6.0 / 1 + \sqrt{(X - 450 / 150)^2 + (Y - 625 / 150)^2}$$

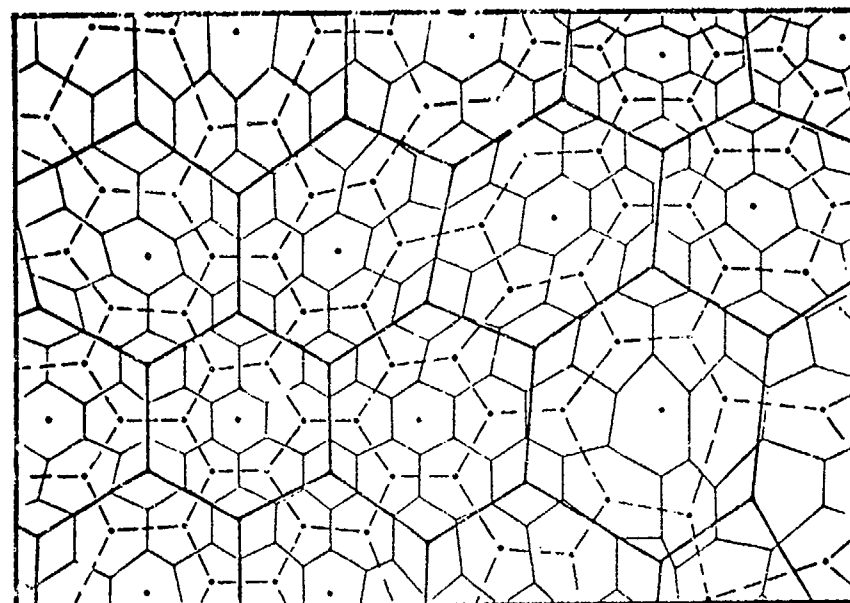
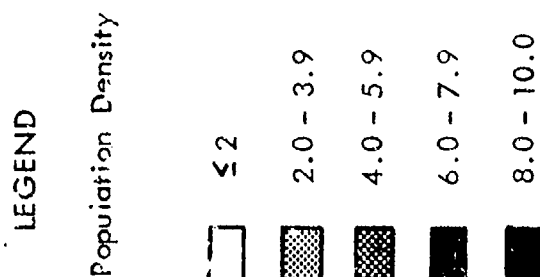
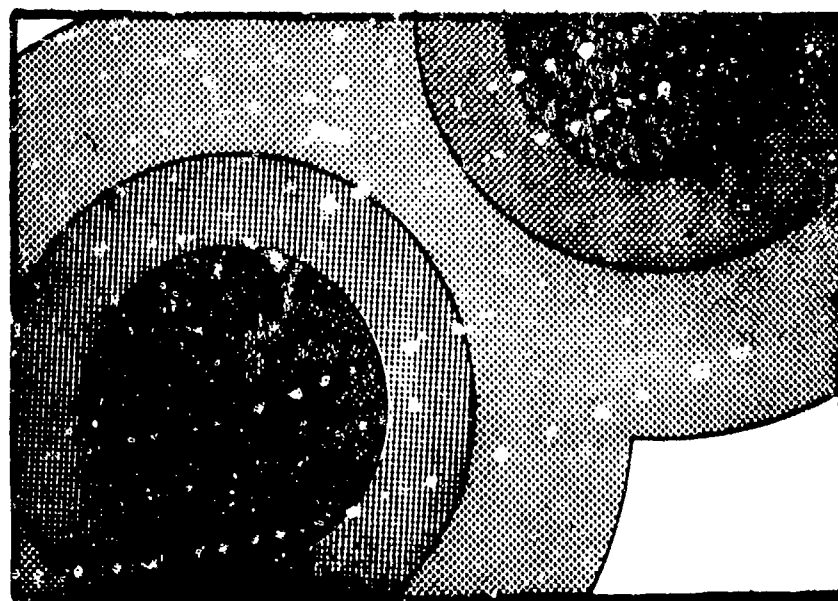


Fig. 35 Distribution of Points After 45 Iterations for Third Trial



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$$z = \max \left[ \left[ \frac{10}{1 + ((x-400)/100)^2} + ((y-780)/100)^2 \right], \left[ \frac{10}{1 + ((x-600)/100)^2} + ((y-550)/100)^2 \right] \right]$$

Fig. 34 Population Density Function for Third Trial

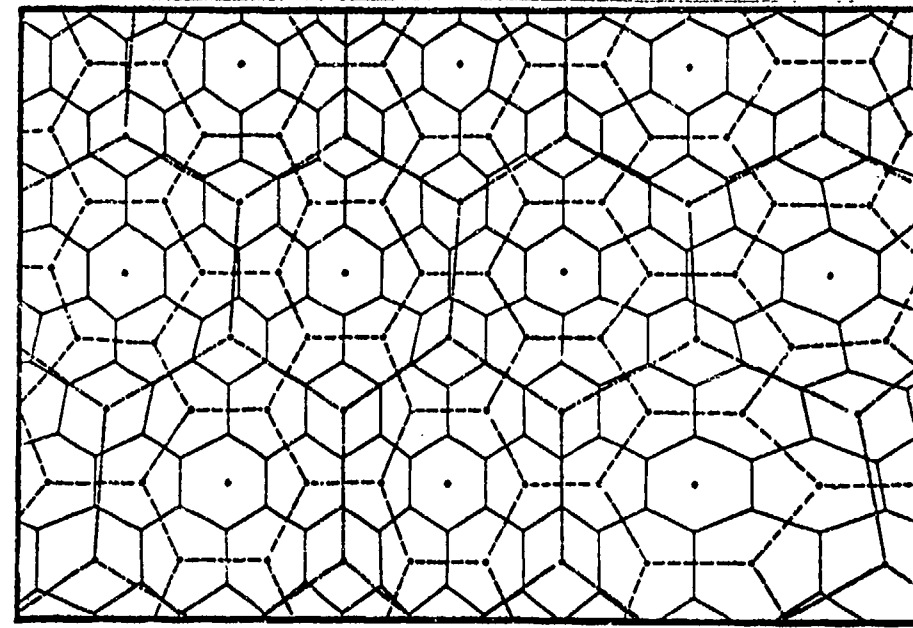


Fig. 37 Distribution of Points After 39 Iterations  
for Fourth Trial

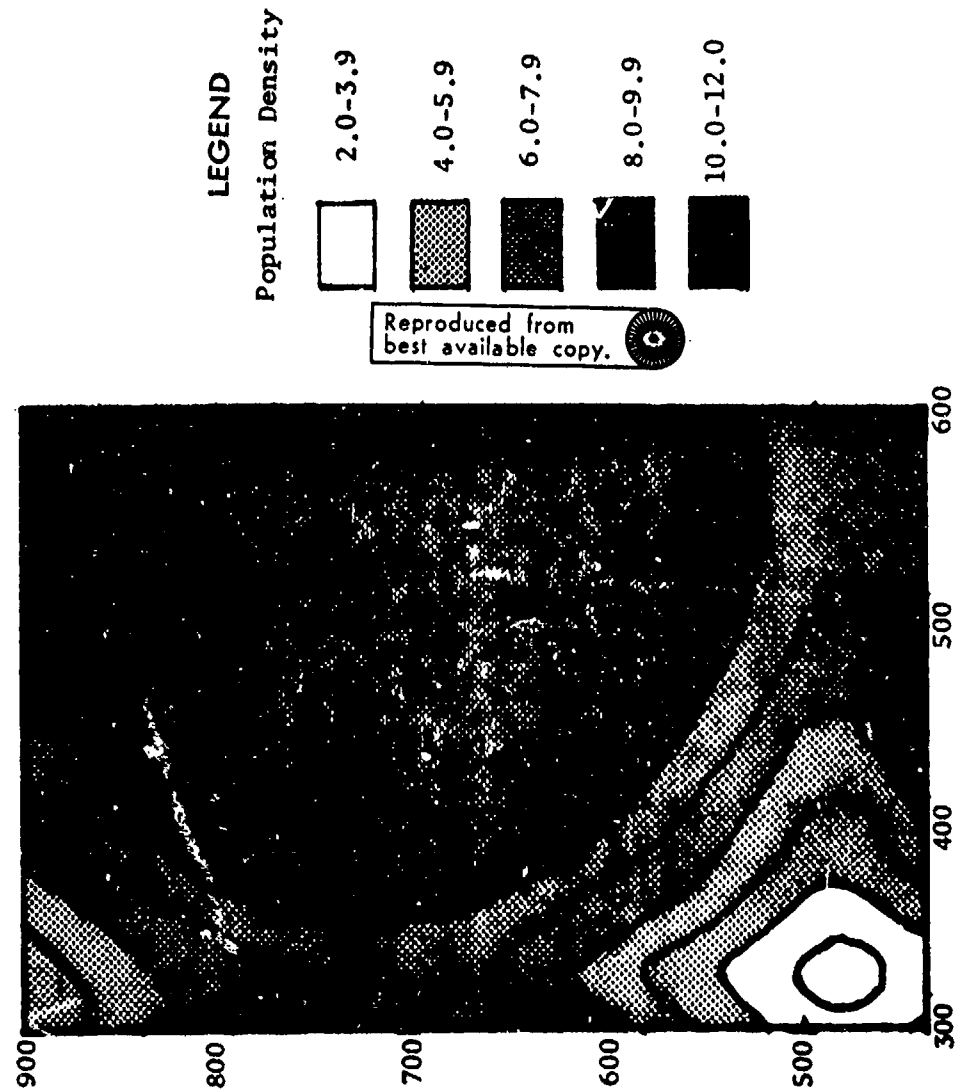


Fig. 36 Population Density Function for Fourth Trial

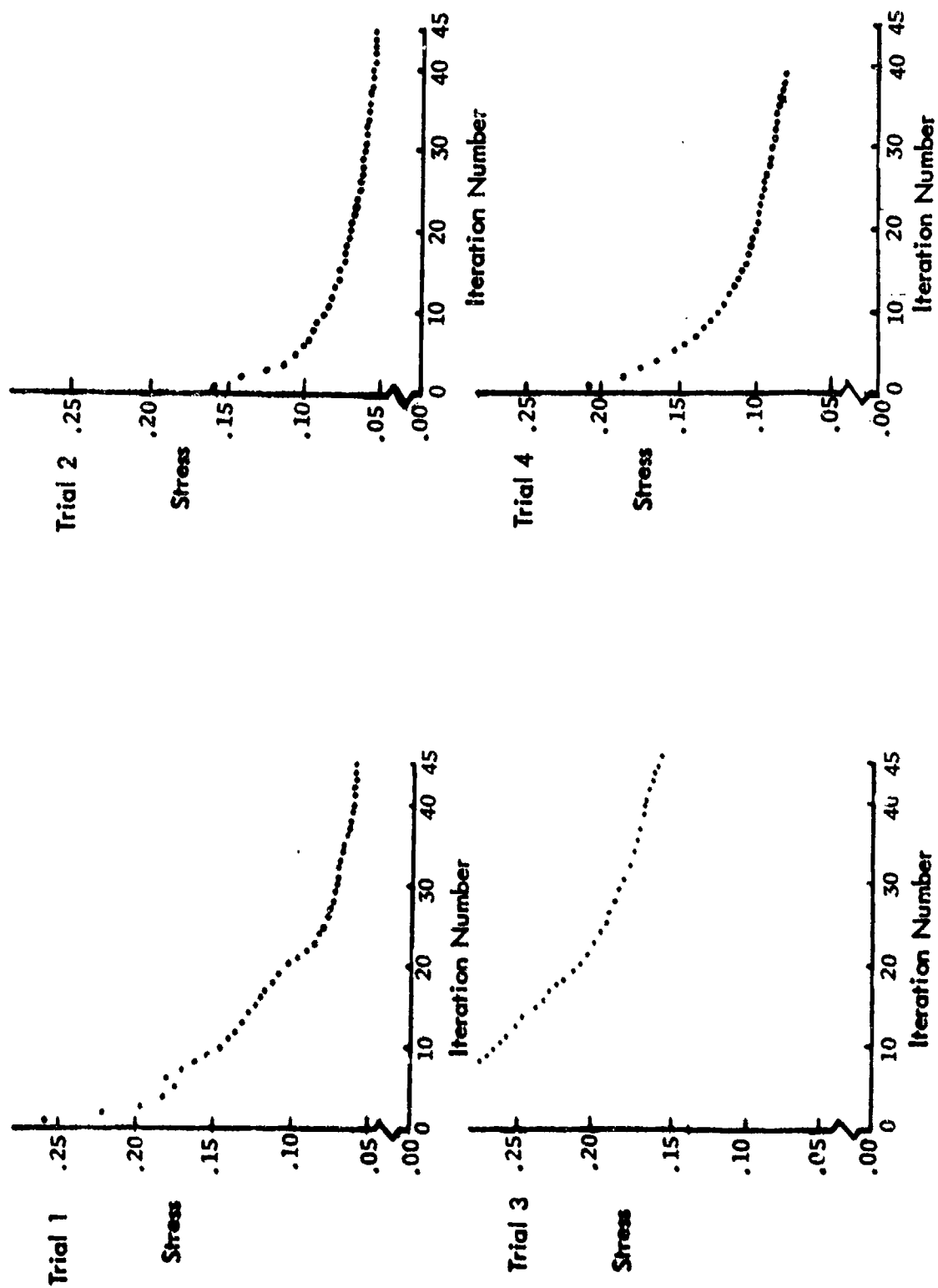


Fig. 38 Change in Stress Values During Iterations on the Four Trials

research strategies in the future. The basic technique used was multi-dimensional scaling - a technique which we have seen was developed quite recently by psychologists and adopted by geographers primarily for study of behavioral problems. A second quantitative thread in the example was the computer modelling strategy of working with a distorted point pattern.

Geographers have gradually acquired expertise in such matters primarily by working with urban forecasting models and with spatial simulations in connection with spatial diffusion studies. Evaluation of the model's goodness-of-fit clearly applied principles from time-honored techniques in descriptive statistics and from the recently developed area of spatial statistics. Finally, the example shows how in solving an old problem in location theory, a model is developed that has many interesting planning implications.

#### Perceptual Studies

Perceptual distance is another area of study that appears eminently suited to analysis by MDS techniques (with simple space output). Two specific examples consist of the Golledge, Briggs, Demko study of intra-urban distances<sup>134</sup> and Whipple and Niedell's study of Black and White Perceptions of Stores in Buffalo (N.Y.).<sup>135</sup>

In the study of intra-urban distances subjects (all located at one point) estimated distances for the  $\frac{n(n-1)}{2}$  pairs of locations selected for the study. The estimates obtained in this way were interpreted as dissimilarities data and the Kruskal IV MDS program was used to produce a configuration based on interpoint distances. Figure 39 shows the configuration of points derived from the subjects' estimates of location. Since the scale of the

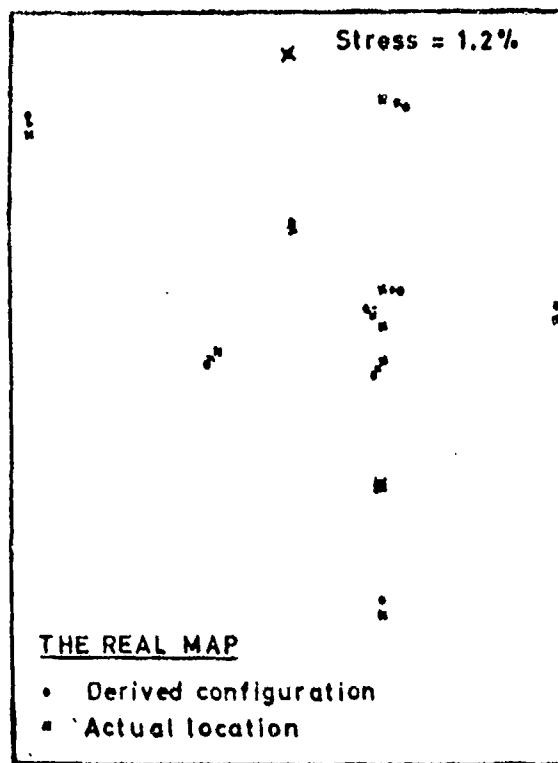


Fig. 39: The actual map is compared to the configuration derived from dissimilarity measures consisting of actual distances between points on the real map.

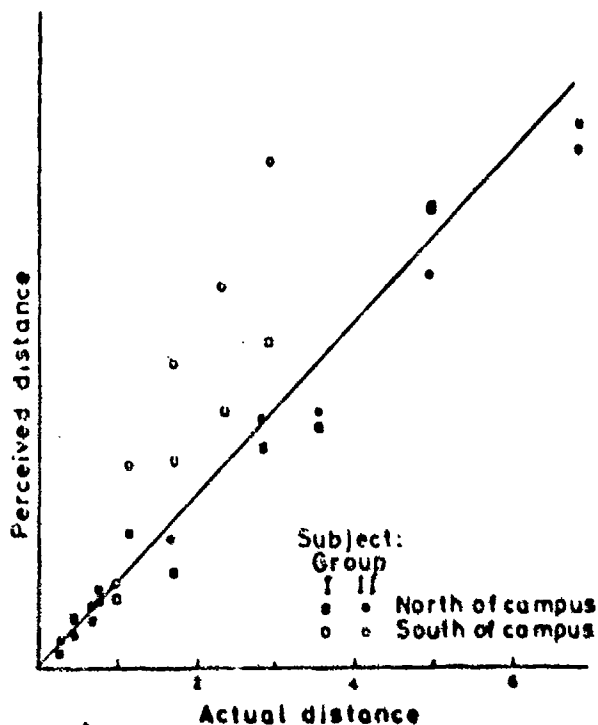


Fig. 40: Perceived distances to places along a major north-south arterial passing through the campus.

Source: R.G. Colledge, R. Briggs and D. Demko, "Configurations of Distances in Intra-Urban Space," Proceedings Association of American Geographers, Volume 1, 1969, pp. 60-65.

analysis was quite small, considerable accuracy of distance estimates was obtained by some sample members. Breaking the whole sample down into two groups based on length of residence, and the distances into subsets "toward the CBD" and "away from CBD" revealed some interesting trends (Fig. 40). For example the newer of the two groups was typified by much poorer estimation of distances, indicating that lack of familiarity with locations distorts the estimates of distance to them and can obviously influence behavior with respect to these locations. Members of the second group improved their ability to locate all features accurately. The variance between the two groups was seen as an indication of different familiarity levels with the city, with corresponding differences in the rates of forming travel habits, and differences in the choices of orientation nodes about which mental images of the urban area were built up.

The other significant feature derived from this analysis was the tendency to exaggerate distances toward the C.B.D. This in turn suggests that increasing congestion (and travel time) tends to increase the perceived distances between places, that the denser packings of land uses around the C.B.D. makes distances appear longer, and individual places harder to locate precisely.

Conclusions drawn from this study were that interpoint distances which are over-estimated probably reduce the likelihood of interaction between points. Suggestions were made as to the likely effect of distorted distance perceptions on things such as places chosen to shop, recreate, and establish residence. It seems reasonable to assume also that further



studies of this type will throw considerable light on the relationships between perceptual accuracy and movement, and the effects that changing configuration (resulting from information changes) have on urban spatial behavior.

Whipple and Niedell's study of black and white perceptions of various stores in Buffalo (N.Y.) provide the geographer with an interesting framework for the analysis of perceptual distances. The authors initially used a semantic differential scale to obtain a ranking of ten department stores (based on "favorableness") by black and white respondents. Frequencies of visit to each store were also collected and again stores were rank ordered for the whole sample. Individual semantic differential scores were transformed to a "distance" measure using the following formula:

$$\left| d_{ij} \right| = \sum_{a=1}^n \left| x_{ia} - x_{ja} \right|$$

where  $\left| d_{ij} \right|$  = absolute distance between a pair of stores

$x_{ia}$  = semantic score of word pair a for store (i)

$x_{ja}$  = semantic score of word pair a for store (j)

The result was a distance matrix of perceived similarities for the  $\frac{n(n-1)}{2}$  pairs of stores - this constituted the basic input to the MDS algorithm.

The results of this analysis were most revealing:

- a) stores that cluster together in the final configuration are more "competitive" than those that are far apart.
- b) black and white perceptions of the favorability of stores varied somewhat but overall the perceptions were quite similar.

- c) both samples did not necessarily shop at the places with the most favorable image.
- d) further study based on social and economic class differences showed very little variation in the perceived favorability of stores.

While the study was undertaken in an integrated neighborhood, and would therefore not exactly mirror variations in perceptions resulting from locational segregation, the methods and results indicate that perceptual distances between competitors may be a useful variable in consumer behavior studies. A modification of this is to find the perceptual distances of stores from consumers (using joint space procedures) would probably be even more useful to the geographer.

A further example of the use of multi-dimensional scaling in the simple space sense is provided by Schwind.<sup>136</sup> Schwind's interest is in the migration distances between states in the United States. The basic input data is dyadic in nature and the algorithm used is the Guttman-Lingoes smallest space analysis program. Schwind generates interregional dissimilarities data on the basis of migrant moves and produces a configuration of the states of the United States in which proximity relations are transformed somewhat on the bases of the migration inputs. Schwind analyzed both dyadic streams of movement (i.e. the net migration rates between every pair of states) and dyadic rates of movement (i.e. the ratio of net dyadic to gross dyadic migration ( $m_{ij} = \frac{M_{ij} - M_{ji}}{M_{ij} + M_{ji}}$ ) where:  $m_{ij}$  is the dyadic ratio, and  $M_{ij}$ ,  $M_{ji}$  represent the directionally oriented flows between any two states i and j).

He concluded that a 3-dimensional solution was most appropriate (by examining the Shepard diagram of the result). Interestingly enough Schwind found dyadic net migration streams to be negatively associated with geographic distance. He argues on the basis of these results that it is justifiable to treat dyadic net streams as similarity data and dyadic net rates as dissimilarity data. The output from his study included: matrices of derived interstate migration distances in a space of specified dimensionality; the geometric coordinates of each state on each recovered dimension; the distance of each point or state to the origin of the  $r$ -dimension space; and graphic presentation of the position of states in the migration space.

Schwind's paper is an interesting one for it emphasizes one of the major problems involved in multi-dimensional scaling analysis -- the interpretation of dimensions. Upon examining the geometric coordinates of states on the recovered dimensions, Schwind argued that the coordinates did not seem to reflect any obvious scaling of states on the bases of income, urbanization, climate and so on. He did not however, attempt to correlate the coordinate values with any scale values derived for the appropriate explanatory variables. He did attempt to interpret the matrices of derived distances of states from the origin of the migration space. Again his intuitive interpretation suggested that "distance to origin" values seemed to suggest that states known for high rates of in-migration are close to the center of a migrant's perception space, and states known for high rates of out-migration are far towards periphery of the migrant's perception space.

Another application of multi-dimensional scaling analysis, this time using a Kruskal-type algorithm, is seen in Gould's analysis of space preference measures with respect to the residential desirability of various states in the United States.<sup>137</sup> Whereas Schwind used net migration rates between states to give him some indication of the similarity and dissimilarity of states, Gould obtained preference orderings of the states from a sample of 25 resident graduate geography students at Pennsylvania State University. As well as obtaining this ordered data, Gould also collected interval-scaled data on the relative advantages of states. Initially Gould considered the point configuration of 51 states in a 2-dimensional Euclidean space. The arrangement of points in this space was interpreted as indicating the similarity of states over the range of subjects. The stress value incidentally was .224 in 2 dimensions. The scaling devices produce interesting clusters of states with perceived similarities. The ordinal scale produced a more circular distribution of points and consequently one that was comparatively easier to interpret. Gould suggested that the configuration resulting from the interval scaled data might be interpreted as a map of America after it had been transformed into "some highly distorted perceptual space." He argued that it was recognizable as a map because states occur close together in geographic space tend to be similarly perceived in terms of residential desirability.

Gould then examined the problem of whether or not the interval scale configuration is simply the ordinal scale configuration which had been randomly disturbed. In order to examine this hypothesis he measured the locational shifts between the interval and ordinal point configurations, collected

them at a common origin (similar to the collection of migratory movements for the purpose of estimating a mean information field), and examined the angles of orientation. His conclusion is that the distribution of angles can be considered as having been drawn from a rectangular distribution. He then regressed an index of social welfare for each state against the perceptual score (scale value) of each state. His conclusions ( $R^2 = 0.61$ ) indicated that the overall mental map of the group reflects the variation of relevant welfare measures to a high degree. Continued analysis of this regression provided some interesting comments on the major residual values in terms of the under-estimation of the images of certain states. Finally, by finding the configuration of individuals in the sample he was able to check to see whether or not individuals located close together in his configuration had similar output configurations for their preference for states. Again he found a high degree of correspondence between the proximity of individuals in the configuration space and the configuration of states in the state configuration space.

One of the significant conclusions from this study was that the ordinal production of data provided a more easily interpretable and clearer configuration than did the more rigorous interval scale data.

#### Archeological Reconstructions

For one final example of simple space output we can again return to the work of Tobler.<sup>138</sup> Using the Guttman-Lingoes smallest space analysis program Tobler produced a map of pre-Hittite cities based on information obtained from analyses of cuneiform tablets. Using some of the assumptions

inherent in the wellknown gravity model, Tobler hypothesized that the more frequently a place was recorded on these tablets the larger would be its size. Furthermore, the more frequently pairs of cities were mentioned together on the same tablet the greater the link between them (either in a trade or spatial sense). Based on these frequency counts he compiled a set of dissimilarity measures and, using them as input, reproduced a configuration of the towns themselves. Since the locations of two of the towns were known he was able to orient his output configurations in terms of latitude and longitude and to suggest an approximate location (within a radius of about 50 miles) for the remaining (and hitherto unlocated) places mentioned on the tablets. The essence of this study is to reproduce an archeological or historico-geographic map of the location of places based on information inputed from a geographic model on their proximities. Information derived from the configuration may possibly then be used to choose among a large variety of alternative locations for archeological expeditions. Incidentally, a similar experiment attempting to reconstruct the locations of former places was conducted by Kendall who used standardized inter-marriage rates for eight parishes in the Otmoor district of Oxfordshire, England between 1600 and 1850 as an index of similarity. Using the Kruskal MD-SCAL program he obtained a very accurate map of Otmoor.<sup>139</sup>

While the above examples by no means exhaust the range of uses for simple space MDE in geography they do give some idea of the types of problems that can be examined and refer to a variety of techniques that can be used in compiling input data.

### Joint Space Output

The use of joint space output appears to have equally as much potential use in geography as does simple space. The essence of joint space output is that both individuals and stimuli are mapped into the same dimensional space. In this way one can obtain the subject preference rankings, and at the same time give metric meaning to the distance apart of individual stimuli. At the same time one can see how close to an individual's "ideal", any particular stimulus comes.

### Inter-urban Migration Choices

One of the more interesting uses of joint space output can be seen in the work of Demko and Briggs in their attempts to operationalize the choice behavior of migrants.<sup>140</sup> They argued that inter-urban migration is the outcome of a choice process in which perceptions of the favorability of each alternative destination is a significant factor influencing final choice. Using a sample of individuals in southeastern Canada, they generated similarities data concerned with the perceptions of alternative urban places and preferential choice data concerned with preferences for these places as migrant destinations.

Each individual was assumed to perceive each city as a union of attribute values. By initially mapping the cities into a perceptual space (based on similarities criteria) each place was given locational and distance characteristics somewhat different to those it possesses in physical (objective) space. In other words, places which have similar combinations of perceived attributes close together in the selected  $r$ -dimensional space, even though they may be far apart in objective space.

The preference model for places is derived from a multi-dimensional unfolding procedure. Individuals rank order places on the basis of "utility" or some other criterion of preference, then the unfolding algorithm defines an ideal point for each individual by unfolding his preferences, and plotting the location of this ideal point in the same r-dimensional space as the similarities data was plotted.<sup>141</sup> Each city then lies a certain distance from each individual and a one-dimensional ordering of the relations between individuals and cities can be compiled. Again additional information can be obtained from the output by clustering the various locations and interpreting which places are likely to have similar drawing powers for given migrant groups.

#### Scaling Space Preference Structures

Rushton has used scaling techniques in an attempt to recover the nature of the underlying tradeoffs between the various stimuli that affect the locational choice of towns for consumer expenditures by a sample of rural households.<sup>142</sup> His approach is described in some detail here since it is both illustrative of the use of the method of paired comparisons in generating a proximity matrix suitable for scaling by non-metric multi-dimensional scaling techniques, and because the four computational steps used in deriving the proximity matrix have been computerized in an integrated computer program.<sup>143</sup> Before describing the four computational stages, however, a brief rationale for the researcher's interest in deriving such a scale is presented.

If our interest lies in hoping to predict spatial choice from a set of alternatives, then we might view observed choices as the outcome of a



perceptual process whereby individuals compare perceived alternatives with an ordering function of all conceivable opportunities in order to judge the most preferred alternative. Returning to reality, it might then be argued that a sensitive treatment of the places chosen -- as compared with those places present, but not chosen, -- might lead to the recovering of the exact degree of substitution of increases in increments of one variable pertaining to the places, for increases in a second variable in the places. Only if such statements can be made -- for all available alternatives -- can we expect to predict choices from unique groups of available alternatives. The analogy with indifference curves in economics and preference structures in psychology has been made elsewhere.<sup>144</sup> In all cases the intent of research is to specify the function that orders all conceivable alternatives open to the individual. Since this function pertains to the relevant stimuli, the basic problem is one of scaling the stimuli.

Computing the input matrix: 1. Definition of Stimuli. Stimuli may be defined at the outset with a sampling or other experimental design arranged so that subjects are constrained to make choices from all alternatives; or subjects might be asked to make choices from objects that are then assigned to stimulus groups. In cases where the researcher's prior knowledge of relevant stimuli is weak, the latter design is more appropriate. Rushton defined stimuli as combinations of distance-separation between people and places, and functional complexity of places (estimated by town population sizes). When one town was chosen in preference to a second, the generalization was made that the stimuli group to which the first place belonged was

revealed preferred to the stimuli-group of the second place. The stimuli groups were called "locational-types". In one analysis, thirty such types were defined.

2. The basic data matrix. From a random sample (603 respondents) of the rural population of Iowa in 1960, information was obtained on the places chosen for expenditures on a number of commodities. Taking the place chosen for the majority of expenditures for groceries, the following matrix was assembled:

Table 11

Basic Data Matrix (hypothetical)

Household ID	Locational Type							
	1	2	3	4	5	6	7 . . . 30	
1	0	*	*	*	0	*	1	0
2	0	0	1	0	0	*	*	*
3	1	*	*	*	0	0	0	*
4	0	*	1	*	0	0	*	0
.								
n	*	*	*	0	0	1	*	0

Legend: Town in the indicated locational type:

- 1 patronized
- \* present, but not patronized
- 0 not present

3. Computing the interpoint probabilities. A measure of the extent of preference for one locational type over another is the probability that one type is chosen over another when both are present and one is chosen. This probability can be computed as a relative frequency by manipulation of the basic data matrix described above.

4. Computing the interpoint proximities. From this measure of the degree of preference a measure of perceptual distance between locational types is required that, at least in an ordinal sense, will indicate closer or further perceptual distance between all pairs of types. For this purpose we use the premise of Cattell that equally often noted differences between stimuli are equal -- unless always or never noticed. Consequently, if one locational type is as frequently preferred to a second type as that type is to the first -- on the occasions when both are present and one of them is chosen, the overall perceptual distance between the two types is zero. The perceived distance between any two types is given by:

$$d_{ij} = |P_{jpi} - 0.5|$$

Read, the perceptual distance from location type i to type j is the absolute value of the difference from 0.5 of the conditional probability that locational type j is preferred to type i when both are present and one is chosen. It follows that  $d_{ij} = d_{ji}$ . A matrix of such perceptual distances is an appropriate input matrix for scaling particularly by non-metric scaling techniques for the quality of the information is such that we are confident only of the rank ordering of the interpoint distances. However, Shepard has shown that the rank ordering of all interpoint distances in a matrix implies the metric position of the points in a space of unknown dimensionality and that, where the number of points is large, (e.g. greater than fifteen), and where the true dimensionality of the space is small, the freedom of movement in this space is most restricted if the rank order of interpoint distances between points in the metric space is to correspond with the rank order in an input

matrix. Hence, he argues, that metric structures are often implicit in ordinal data.<sup>145</sup>

Scaling the locational types. The locational types were scaled by the method of Kruskal<sup>146</sup> and a graph of the scale values for the one-dimensional solution are shown in Figure 41.

Significance of the recovered scale. The significance of the recovered scale lies in the fact that, while it was "measured" from observed behavior in a spatial system of opportunities, we yet have reason to believe that it might explain spatial choice in a region where the density and arrangement of spatial opportunities is different.<sup>147</sup> Preference scales are fundamental descriptions of behavior in the sense that they show how all hypothetical alternatives are evaluated. Since a particular environment is a unique subset of the set of hypothetical alternatives, the preference scale may be used to evaluate this special case. It is this generality which an appropriately designed preference scale possesses that leads to its great potential in solving research problems.

Interpersonal comparisons of scales. Ewing, in still unpublished research, has compared the preference scales of locational types for different social and economic groups of the Iowa households.<sup>148</sup> He has applied significance tests to the differences in probability values (see step three above) in the cells of an input matrix and he has also used differences in scale values for a subset of alternatives to compute the probabilities of interaction with any alternative. He found that the greatest difference between preference structures was between groups of households one of which had

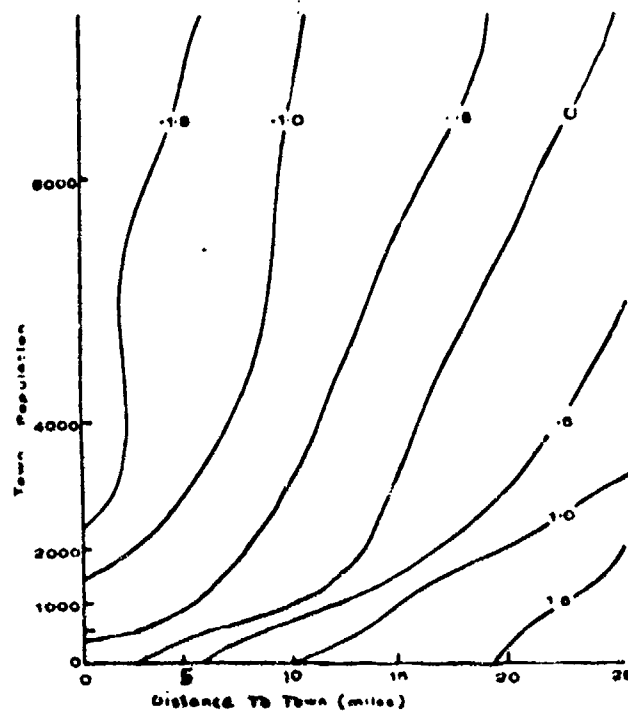


Fig. 41: Space Preference Structure for Grocery Purchases:  
Iowa 1960

been shown to patronize the nearest available opportunity while the other group was composed of households who by-passed the nearest opportunity in favor of some other. This result may be contrasted with that of Ermuth<sup>149</sup> who found no difference between the preference scale of one group of urban households who claimed (in a test question using the semantic differential) that the distance of a store was important in their choice, and the scale of a second group who claimed that distance was not important in their choice.

Temporal comparisons of scales. Several factors lead to temporal changes in such scales. Changes in how people evaluate alternatives often induced by changes in the character of the alternatives themselves and in the transportation system that relates them to the alternatives, may lead to changed preference scales. One study has compared the Iowa preference scale for grocery purchases in 1960 with that of 1935.<sup>150</sup> Major differences were found in the two scales that can be summarized as an increasing tendency in 1960, for Iowa households to by-pass small towns at close distances for larger towns further away. Of course, such generalization, has been made before on the basis of less formal and less quantitative research; but the precise calibration of the change is not possible unless the scales are computed. The growth or decline of specific towns in this period will depend to a large degree on their position in relation to the two scales.

Joint-space analysis of preference scales. A second way of approaching the question of choice in different environments is to ask whether spatial choices for like things in different areas can be regarded as different points of view from which the two groups evaluate the stimuli (locational types). Beginning

with a matrix consisting of the scale rankings of the locational types for three commodities in Iowa and for the same three in Michigan, the six sets of rankings were evaluated to determine where each was positioned in the joint space with the thirty stimuli. The two-dimensional joint-space solution is shown in Figure 42. The stress value which measures the goodness-of-fit is 0.052. The close proximity in this preference space of five of the six points representing the groups shows that systematic differences in viewpoints of those five groups do not exist, rather the ordering of spatial alternatives is similar in all five cases. The sixth point, describing the viewpoint of the Michigan group choosing towns for clothing purchases, is anomalous and deserves further study. Such differences in preference structures can be attributed to one or both of two sources. They may indicate that one group evaluates similar stimuli differently; for example the results above might mean that Iowa households have a greater propensity to patronize small, local towns whereas their Michigan counterparts might have changed their former habits and now prefer to make the longer journey to the bigger towns. Alternatively, the different preference structures might reflect the fact that similar-sized towns in Michigan and Iowa might contain different amounts and types of clothing stores and then the observed pattern would be a reflection of the ambiguity present in the surrogate variable "town-size" as a measure of town content. Further research would clarify these interpretations. However, the anomalous group aside, the tight cluster of the other five groups in the perceptual space and the accuracy with which the independently computed preference structures could be recovered from this joint space,

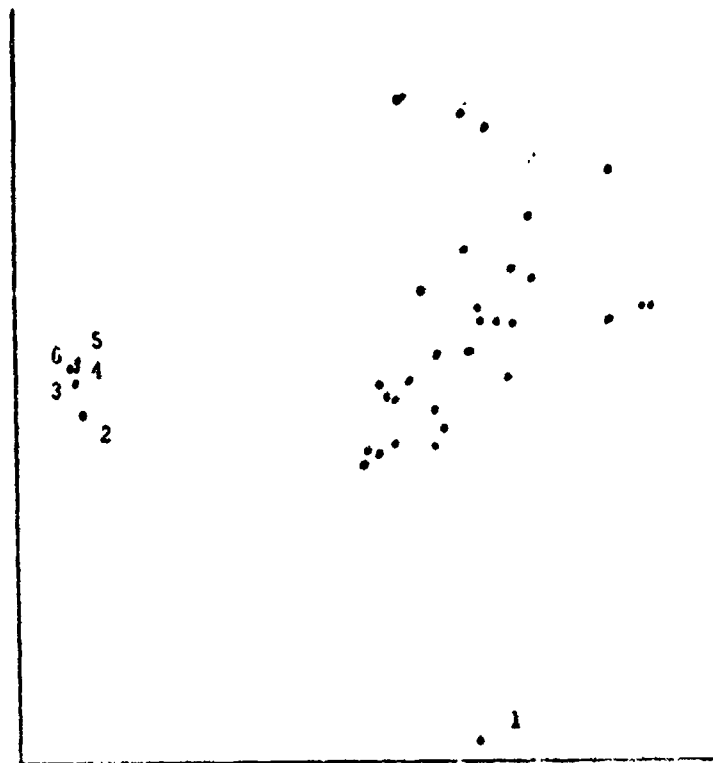


Fig. 42: Joint-Space for Consumer Spatial Choices in Michigan and Iowa.

Legend: 30 unidentified points refer to the 30 locational types.

Points numbered:	1 - Michigan, clothing
	2 - Iowa, clothing
	3 - Michigan, appliances
	4 - Iowa, appliances
	5 - Michigan, groceries
	6 - Iowa, groceries



is an indication of the consistency of spatial-preference structures for different trip purposes as well as for choice of towns in different areas.

An ingenious recent application of joint-space analysis was designed to shed light on the problem of interpreting scaled dimensions. In a conventional study of people's ranking of U.S. states for residential desirability, Lieber<sup>151</sup> added to the  $m \times n$  matrix of  $m$  states and  $n$  state viewpoints,  $1 \times m$  objective measures of the states on variables hypothesized to be related to residential desirability preferences. He then simultaneously scaled the  $m + n + 1$  points in the same space and interpreted inter-point distances between the state viewpoints and the objective criteria as a measure of the degree to which the state viewpoint corresponded with the various objective criteria.

Incorporation of preference scales in diffusion models. De Temple has argued that a space-preference structure is a more appropriate predictor of spatial interaction rates than the more commonly used distance-decay functions, since, as we argued above, the preference structure is more sensitive to the unique distribution of people or places in a given context.<sup>152</sup> He used preference structures for towns selected for different commodity expenditures -- generalized for the probabilistic allocation rule of Luce in order to generate probabilities of interaction with places -- to govern the spread of the adoption of a farming innovation. Formerly, distance-interaction rates had been used for this purpose. His operationalization of the diffusion model is thus closer to the theoretical model first proposed by Hägerstrand.<sup>153</sup> Others experimenting with preference structures include Ewing<sup>154</sup> and Girt<sup>155</sup> who

have extended the application of space-preference structures by applying Luce's choice axiom, and Briggs<sup>156</sup> in his study of preferences for shopping centers in Columbus, Ohio.

Incorporation of preference scales in central-place theory. Since central-place theory describes the theoretical location of business clusters (settlements) resulting from the mutually adaptive behavior patterns of entrepreneurs and consumers, scales that describe how one of these groups responds to actions of the other group ought to be the fundamental postulates of the theory. But the scales that were used in the classical statements were so simple--and naive--that they were not commonly recognized as scales. Thus, from the consumer's point of view, the postulate of Christaller that the consumer would patronize the closest place offering the required good was essentially a scaling of relevant alternatives on the distance variable and postulating that the alternative with minimum distance scale value would be most preferred and hence patronized. Described in this way, it would seem to be a natural development for the theoretician to question the effect on the derived patterns of settlements of substituting more realistic consumer preference scales for the one by Christaller and implicitly accepted by most researchers who have "tested" the theory since his work first appeared. A formal model of central-place theory has been constructed in which the consumer preference scale has been varied.<sup>157</sup> The results were that, accounting all the other postulates, constraints, and environmental assumptions of Christaller, the grouping of unique bundles of goods in a central-place hierarchy was no longer a derived property of the theoretical system. Such findings support

the thesis that preference scales are an important input to location theories for mutually adaptive behavior patterns are a hallmark of all location theories.

Further evidence of the usefulness of such preferences in substantive research is found in other (unpublished) work at The University of Iowa. Bell has used trend surface equations of the scale values in a computer program that estimates tributary populations for market centers. From a close checkerboard sample of rural locations, the algorithm evaluates surrounding towns and allocates the area surrounding the sample point to the most preferred market center, using the equation for the preference function for the activity in question. He is currently comparing the relative sizes of tributary populations surrounding centers from which activities have left in the past decade with centers that have supported the activity through the decade and centers that have added the activity to their business structure in that period.

### Conclusions

The serious attempts currently underway to integrate the disparate findings from behavioral analyses in geography may be contrasted with the simple--possibly naive--approaches of the classical theorists. It is no longer possible to separate current geographical research into purely deductive theoretical approaches and inductive, empirical approaches as was possible ten years ago. For today, the empirical findings of micro studies of behavior have frequently become the foundations on which theory is built. Conversely, the models--frequently computerized--of the abstract theoretical systems are increasingly being taken and connected to observational data.

This interaction between theorists and empiricists augurs well for future advances in the discipline. As theorists become more aware of this interaction, they become more concerned with the problems that the empiricist encounters in constructing models of the theory and in operationalizing them with respect to available data. It is in these terms that we understand the current trend to construct theory for the variegated environment - anisotropic plain. In all of this endeavor, the modern computer is both a pre-requisite and, indeed, has functioned as a spur to scholars to think in directions that previously were unthinkable.

As with any "current trend" many "loose-ends" remain. The search for acceptable behavioral foundations is in its infancy and even the basic logic in which these foundations will be couched is being actively debated at the present time.<sup>158</sup> Decision - models have been described and the discipline has been urged to endorse several of them.<sup>159</sup> However, at this point not one has been effectively operationalized and applied. Presumably the future will provide these examples and, in this context, the description of measurement problems contained in this chapter and the associated discussion of multi-dimensional scaling will soon be rendered largely obsolete as the cutting edge of the discipline advances.

In contrast with the integrative approach to theory construction of the classical studies, current work has a propensity to stay for a long time with the analysis of small subsystems of reality. The grand models that will integrate these diverse studies have yet to be written and, when they do appear, they are likely to appear unduly simplified. Frequently this will

result from the undeveloped nature of the data base being used but frequently too it will result from the complexity of the system being modelled. The focus of both geographical research and location theory on spatial differences remains, but the paradox of the quantitative era is that it was ushered in by Schaefer's paper on exceptionalism (see Chapter II)<sup>160</sup> which insisted on the search for spatial order by the methods of contemporary science, and, without further apparent revolution its contemporary heritage is a viewpoint that models spatial activity as the operation of complex processes (physical or human) on "complicated" environments--thus conceiving of reality as an ordered flux of unique events. The task is great, progress may be slow, and Wells' dictum rings true: "The forceps of our minds are clumsy forceps and crush the truth a little in taking hold of it".

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## CHAPTER VII

### PREDICTIVE MODELS IN GEOGRAPHY

True scholars are often loath to address themselves to problems and research which falls under the general rubric of predictive models. In many instances geographers have dealt with both inductive and deductive models which are in fact, or could very easily be reformulated into, predictive models. Yet, most do not clearly identify that possibility or they fail to perform the tasks necessary for the transformation. A conservative view might be that geographers currently have not completed the research efforts necessary for valid predictive models and thus shy away from such efforts. An alternative explanation, and perhaps the more correct one, is that the geographer's research very often focuses on establishing narrowly defined relationships, forming a single element or component of the system, rather than attempting system-wide models which are more consistent with the need for predictive models.<sup>1</sup>

It is the purpose of this chapter to discuss predictive models in geography. Several of the models referred to have not been formulated in such a manner so as to allow prediction directly but clearly such reformulation is possible.

#### Inputs necessary to the development of valid predictive models

Before identifying specific models in the literature dealing with spatial phenomenon, it is important to note and discuss those elements which are necessary for the development of viable predictive models.

# 1. Theory

Some would argue that a strong theoretical base is not necessary in the development of valid predictive models. This argument has been discussed in the geographic literature, as elsewhere, primarily with respect to the difference between explanation and prediction.

## a. Prediction - Explanation Symmetry

As regards the importance of explanation being a necessary component of prediction, two points of view exist. The noted Iowa philosopher, Bergmann and many of his philosophy of science colleagues would maintain that there is logical symmetry between explanation and prediction.<sup>2</sup> Bergmann states the logical requirements: (1) a closed system (or control boundary conditions), (2) a complete set of relevant variables, and (3) a notion of interaction between them. A recent methodological statement by Golledge and Amadeo is in general agreement with this view.<sup>3</sup>

Scriven and Hanson suggest that explanation and prediction may be considered as completely independent pursuits.<sup>4</sup> King has suggested in a recent paper that geographers have been overly concerned with explanation and as a result have avoided important lines of research since adequate explanatory models were not available.<sup>5</sup> Blalock has presented an interesting idea in arguing that, because of the inherent differences between the "languages of theory and research", theory cannot be completely relied upon to provide the bases for predictive models:<sup>6</sup> we "think" in terms of theoretical language that contains notions such as causes, forces, systems and properties, while we "test" in terms of covariations, operations and

pointer readings.

b. Modeling and science

A model may be constructed directly from theory in the form of a symbolic statement of that theory's relationships. George, in his work on the use of models in science has called this process of reducing the language of theory to its basic logic "formalization," while Chorley has labeled it translation.<sup>7</sup> In a more pragmatic statement Kilbridge, et. al., state that "the underlying theory... is that set of relationships, stated or implied, which is soon to prevail between the subject (activity, entity projected) of the model and the larger environment."<sup>8</sup>

Theory may be used as the underlying basis for the modeling process in several ways. Such has been the case with such concepts as the gravity principle, that is the concept of systematic distance-decay in the probabilities of expecting spatial interaction; market analysis, that is, the concept of market boundaries conforming to the location of equal delivered prices (cost at site plus f.o.b.); and location theory, such as the concept from central place theory that entrepreneurs locate so as to command equal-sized (threshold) market areas. A specific example of the use of a theorem as the basis for an operational heuristic model is Graybeal's simulation model for residential development which matches the supply and demand for housing.<sup>9</sup> A second example is Alonzo's model of residential distribution where location of residential activity was derived through his translation of classical economic theory into a form applicable to a particular spatial context.<sup>10</sup> Finally, specific concepts embedded in theoretical formulations may be used as the

basis for developing alternative model formulations. For example, Rushton, Golledge, and Clark use the basic concept of distance minimization by consumers as defined in central place theory and develop a model based on notions concerning a view of consumer behavior more in keeping with deviations from that concept that are revealed when empirically analyzing such behavior.<sup>11</sup>

A second approach to the development of predictive models in geography, and in fact the social and behavioral sciences in general, is the utilization of empirical regularities. Since many of the theories of the social and behavioral sciences are not sufficiently explicit it is possible in many instances to attribute alternative theoretical bases to empirically derived relationships.<sup>12</sup> The relationships are causally ordered, usually, in an ad hoc, eclectic and pragmatic way, often dependent on the value system of the researcher.<sup>13</sup> In some instances an econometric or mathematical technique may provide the model structure, sometimes without any regard to generality or logical continuity between the observed regularity and the solution methodology, i.e. a linear statistical framework may be utilized for the model when a mathematical model might be more appropriate.

A third approach to the development of predictive models severs all relationship with theory.<sup>14</sup> For instance, the direct extrapolation of a curve, prediction of group membership on the basis of discriminant analysis, and many applications of Markov chain analysis to specific problems are representative of this class. The main difference between this and the previous approach is conceptual rather than operational.

## 2. Operationalization and data

Given that a model builder has accepted or rejected a theoretical base for his particular problem, and designed a logical framework appropriate to encompass his objectives, the actual construction of the model must be undertaken.

In general, three major model elements may be identified: (1) components, (2) variables, and (3) relationships.<sup>15</sup> A component may be defined as the entity or activity that is to be projected, allocated or manipulated by the model. For example, in an urban context components may be land use, transportation, population, and/or economic activity. Given that the researcher has chosen his observational entities he then must decide upon the dimensions of the model as they relate to comprehensiveness, degree of aggregation, dynamics, and the treatment of time.

If the modeler has chosen a specific theoretical concept, this framework may provide for clear cut definition of the model dimensions. However, in many instances, these decisions are determined because of constraints from the data base. In general, specification of model detail are of three types: (1) spatial disaggregation (grid versus point locations), (2) functional or sectoral disaggregation (hierarchies of activities, observational entities by attribute), and (3) temporal disaggregations (continuous versus discrete). Clearly, one of the major difficulties in the development of predictive models is the lack of adequate data with these characteristics for spatial analysis.

A predictive model by definition usually purports to represent the outcome of a specific process within some specified temporal dimension. In

other words, given the state of the system at time  $t$  and some priming action, it is possible to specify the state of the system at some time  $t + n$ . It should be noted that the time dimension is of extreme significance. The modeler may choose from a range of choices beginning with comparative statics to real time monitoring and simulation of the process.

Variables and attributes are descriptors which indicate the status of the components and may be either exogenous or endogenous. The exogenous variables may be either controllable, in which case they are called policy decision variables, or considered uncontrollable, and accepted as given. Endogenous variables may be divided into output and status variables. This distinction is somewhat relative in the sense that status variables describe the state of the system in any moment and in effect may become output variables at the completion of the model operation.

Model relationships are in simplest terms linkages between the variables and usually are represented as equations or sequence of operations. An algorithm or solution method provides the means for producing an output vector signifying the state of the system at the future time period  $t + n$ . In an urban context, Lowry has indicated that models with a tight logical structure and uncomplicated linear functional relationships lend themselves to analytical and recursive solutions.<sup>16</sup> Predictive models which lack logical closure often require an iterative solution procedure.

Depending upon the tractability of the problem, one of three modeling methodologies is usually selected. One choice is an econometric predictive model requiring an estimate of structural relationships. A problem with



this method is that relevant variables are often highly correlated.<sup>17</sup> A second solution method is mathematical programming in which the model optimizes an objective function subject to a series of constraints.<sup>18</sup> A third methodology is simulation which in many instances is used when a solution by analytical iterative methods is difficult or intractable.<sup>19</sup> Simulation methods directly incorporate change, random fluctuation, and probability distributions describing the various events which occur in the system.<sup>20</sup>

### 3. Testing Models

Models used in geography, as elsewhere, must be calibrated prior to testing. The variables utilized in the model must be given precise empirical definition and numerical values must be provided for the model's parameters. At this stage, often the modeler must revise and/or reconstruct portions of the logical model structure to reduce its sensitivity to bad data or to make better use of what data is available.<sup>21</sup>

Obviously, the appropriate test for a predictive model is to utilize it as intended and compare the forecasted outcome with reality at a future point in time. Since the model is predictive it is clear that the data is not available for testing in this way and thus an ex post facto test is usually necessary. That is, take the state of the modeled system at time  $t - n$ , calibrate and apply the model for time  $t$  and compare resulting values to the observed values. Admittedly, this is an imprecise test at best, but in fact, the paucity of data for past time periods often precludes even this minimal attempt at testing.

Alternatives to the performance test include "sensitivity tests" and

"contingency analysis." While neither test the model's predictive accuracy they test the quality of the model's design.<sup>22</sup> As used to test the goodness of design, sensitivity analysis is applied by varying the values of key parameters to assess how responsive the results are to such variation. Contingency analysis, in contrast, requires that the assumptions basic to the model are varied in order to evaluate outcomes given changes in these basic conceptual building blocks.

#### Constraints in the Development of Models in Geography

Predictive models have not been developed to any degree in the geographic literature and, in fact, are only addressed in a few recent articles. Geographers appear to be little concerned with the development of predictive models, reflecting the reluctance of the discipline to approach applied problems amenable to geographic analyses. Constraints in the development of models per se in geography are numerous and might help to explain this situation. Hence, an attempt will be made to isolate several of the major obstructions to model development.

##### 1. Theory Construction versus Philosophy

If we take the position that the development of theory, although not a pre-requisite, is nevertheless a substantial aid in constructing predictive models, geography's failure to develop an expanding body of theory must be considered one of the major deterrents to model development. Since the turn of the century American geographers, in the process of emancipation from European influences, devoted a great deal of energy to very illiterate, but often fruitless, dogmatic debates. Semple, in numerous articles between 1897 and 1925,

detailed influences of geographical environment on man.<sup>23</sup> As a student of Ratzel's, she defined geography as a study of cause and effect and attempted to show how individuals are influenced by their natural environment. Davis' introduction of the term ontography, which "...is meant to include all the responses of organic forms for their physical environment, with their physiological and individual behavior or in racial habits...", furthered the cause of the environmental determinists.<sup>24</sup> Huntington, in his 1923 presidential address on "Geography and Natural Selection," which was modeled closely after the evolutionary theories of Darwin and Spencer, emphasized the biological and cultural selection and survival in the process of expansion and migrations of cultures.<sup>25</sup>

However, Harvey has reached the general conclusion that determinism is not a hypothesis in the ordinary sense at all and, in fact, cannot be subjected to direct or indirect empirical testing.<sup>26</sup> As such, it is basically a working assumption. Thus, Harvey states that the major methodological dispute in geography over determinism arose out of the failure to distinguish between an a priori model which has no empirical justification and a theory about reality. Elsewhere he notes that while scientific theory may be evolved in two different ways, the theoretical deductive approach is now most favored in the sciences since it recognizes the hypothetical nature of scientific thinking. Nonetheless, this approach has not been prevalent in geography.

The loose paradigm of "environmentalism" was based upon presumed analogies in deductive reasoning between the physical and human domains. The methodology applied to human geography under the banner of environmentalism

was basically a search for situations in which the environment determined human activities. While the application of very strict cause and effect principles in physical geography may be somewhat justified in the sense that precise physical laws are controlling processes, human geography must be considered in a different light, given its objectives and focus. The application of environmentalism in human geography results in a greatly simplified, monocausal and genetic explanation.<sup>27</sup>

In addition to environmentalism, a thrust of Hartshorne's philosophic statement, based on the premise of areal uniqueness, did a great deal to stem the development of theory. As Harvey indicates, it appears the Hartshornian orthodoxy progresses from the study of unordered observations or facts through classification and generalization to the development of general statements and principles.<sup>28</sup> These, then, may be used to assist in the explanatory description through the idiographic method of unique areas. Uniqueness in and of itself is not a handicap, for clearly the uniqueness of different areas is not debated. However, the search for uniqueness rather than the search for similarity has been a misdirection and deterred geographers from developing a more general body of theory.

## 2. Difficulties of Dealing with Spatial Data

Any geographic research must deal with either locational or areal data of some sort. Locational data are geodetic coordinates of a small point on the earth with respect to some arbitrarily chosen reference system as well as any other form of information which can be associated with that point. Areal data are those which characterize some defined region on the globe. Writing

on the geographic ordering of information, Tobler classifies three types of geographic data:<sup>29</sup>

- (1) arbitrarily coded data with a list of characteristics,
- (2) geographic areas with associated information, and
- (3) bench mark systems.

One of the major difficulties facing geographic research is the acquisition of data which is spatially coded. This is particularly true when attempting to develop a longitudinal series of spatially coded data. The geographer attempting to do meaningful research usually must put forth considerable effort in developing adequate spatial data. The number of interesting topics that have been altered or abandoned, because of this deterrent, goes uncounted.

While the introduction of computer technology has been an enormous aid to geographic analysis, it has also forced the geographer to order and record information in a quantitative manner so as to facilitate computer analysis, rather than storing it in a descriptive way which could more easily lead to alternative subjectively based conclusions.<sup>30</sup> However, little progress has been made in the development of languages or other procedures specifically designed to deal with spatial data.

The definition and acquisition of spatial data is not all that plague geographers with respect to useful information. Observational entities provide another set of problems.

The areal differentiation and covariation approaches in geography are based on the concept of a population or elements of items distributed in space. A population consists of a spatially delimited aggregate of items or elements

which conform to a given definition and each has a definite location at a given point in time. Since each item has a uniquely defined location, the population has a distribution in space consisting of the aggregate of individual locations. When the elements become too numerous and it becomes inconvenient to specify their individual locations, the distribution is normally described in terms of an allocation of the items among areal units.

Considerable methodological problems occur when attempting to develop inferences about relationships based on areal data. In his "classic" paper, Robinson categorically states that such inferences are not justifiable; individual relationships inferred from areal correlations may be seriously biased in magnitude and erroneous in direction.<sup>31</sup>

Duncan points out that the problem of individual (point) versus areal (cell) correlations is similar to the problem of aggregation in economics between macro and micro studies.<sup>32</sup> McCarty, *et. al.*, speaking about a problem of "scale" or "areal levels of generalization" conclude that studies made at one scale (their example, townships) do not permit that conclusions be applied to the problem as they are expressed at other scales (their example, counties).<sup>33</sup>

The work of Cliff and Ord as well as others concerning the problem of areal auto-correlation are beginning to make substantial contributions to our understanding of the statistical problems related to the utilization of areal units as observational entities.<sup>34</sup> However, in many instances in both applied and basic research these very difficult problems are assumed away and taint much of the geographer's literature.

### Forecasting and Predictive Models in Geography

This section focuses on a variety of research facets in the discipline of geography. In some cases the models discussed below may have been developed outside of geography yet are particularly pertinent to the interests of the discipline. That is, they treat directly spatial aspects of growth and change. Also in some cases, the models discussed were not developed primarily for predictive purposes, but rather to identify certain empirical relationships which exist. In each case, however, the models discussed may be reformulated in order to function as predicting or forecasting models.

#### 1. Diffusion of Innovation

Early research examining diffusion of innovation was characterized by a strong bias toward an empirical inductive approach emphasizing landscape development over a considerable period of time.<sup>35</sup> A major focus of this research was on describing the extent of the diffusion, the item being diffused, and the landscape, rather than on understanding the specific processes by which the phenomenon, or acceptance thereof, moves from one location to another.

The second line of research in the diffusion of innovation characterized by Hägerstrand's work focuses on the locational aspects of the diffusion of innovation.<sup>36</sup> This more deductive approach focuses on the generative processes. The utility of spatial diffusion models as a predictive device was alluded to by Hägerstrand: "the spatial order in the adoption of innovations is very often so striking that it is tempting to try to produce theoretical models which simulate the process and eventually make certain predictions

achievable."<sup>37</sup>

No attempt will be made here to outline the structural properties of either the Hägerstrand or related models. Instead, literature will be cited to show that only minor reformulation of the scope and the objective of many diffusion models converts them into predictive models.

While at first glance, diffusion of innovation models would appear to offer ex post facto predictions alone, this need not necessarily be so. For instance, Bowden's studies of the diffusion of irrigation wells in Colorado projects the spatial distribution of wells up to 1990.<sup>38</sup> Bowden follows Hägerstrand's conceptualization and operationalization quite closely. He calibrates his model with data from the period 1935 to 1963. In testing his model for that time period Bowden found a close correspondence between the simulated and observed patterns of diffusion. Encouraged by this tautological prediction, he then used the model to project the future distribution of wells and estimate its impact on ground water supplies under alternative conditions.

In general, geographic efforts following Hägerstrand's methodology focus upon the processes, operating in space and time, which generate an observed distribution of phenomenon. Researchers generally select a set of critical variables relevant to the diffusion or location processes, relate these within a model which imitates the real world process and hence hope to generate patterns similar to real world patterns. The generated patterns are assessed and compared. Evaluation usually takes the form of manipulating the values of variables and parameters used in the structural relationships



to assess the sensitivity of the generated patterns to such changes.

Application of the location oriented diffusion principles are many and include Bowden, Brown, Pederson, Hägerstrand, Pyle, and Colenutt.<sup>39</sup>

Morrill's varied applications of diffusion analysis to the spread of urban blight, the movement location of urban activities at the periphery of the urban areas, and the expansion of urban ghettos, particularly the manner in which predictions can be made of future movements of Negroes in Seattle, Washington are all indicative of ways in which diffusion of innovation work can be converted into forecasting models.<sup>40</sup> A further example is Bylund's study in Sweden which models the spread of colonization and the frontier area through the use of spatial diffusion principles.<sup>41</sup>

A somewhat different approach is that of Hudson.<sup>42</sup> Addressing the problem of predicting interaction in a Christaller-like central place system, he develops a stochastic model whose parameters are determined by the number of levels in the hierarchy and the spacing coefficients of the centers. The latter approach appears to provide an adequate prediction of the diffusion of several innovations.

## 2. Migration Models

Migration models attempt to define relationships between place and distance variables and migration levels. Thus, migration models, except in some few cases, may be considered predictive models. In general such models are calibrated upon the development of a conceptual framework and migrations are predicted ex post facto, although some exceptions should be noted.<sup>43</sup>

Classical models developed in sociology such as those by Stouffer and Zipf rely substantially upon gravity formulations.<sup>44</sup> Zipf gives reasons why intercity movements for goods and persons between two communities will be directly proportional to the product of their population and inversely proportional to the distance separating them. Stouffer proposed an alternative conceptual framework based on the premise that: "the number of persons going a given distance is directly proportional to the number of opportunities at that distance and inversely proportional to the number of intervening opportunities."<sup>45</sup>

Everett Lee's work, which is a restatement of Ravenstein's "theory," contributed a new definition of migration which recognizes the importance of internal migration and spurred further research.<sup>46</sup> Moreover, it emphasized that distances are omnipresent but not omnipotent and it identifies other factors such as the perception of conditions at both the origin and destination, and personal sensitivities that are related to migration streams. Variables such as these are becoming increasingly important in a variety of models being pursued in the geographic discipline. It should be noted that geographers in increasing numbers are adding new dimensions to models by broadening their definition of process to include relationships beyond those of space and time.<sup>47</sup>

Some sociologists have emphasized temporal dimensions, such as Taeuber, while others, Eldridge for example, follow a demographic approach.<sup>48</sup> Geographers have consistently attempted to integrate both spatial and temporal dimensions.<sup>49</sup> While earlier research into migration emphasized

the gravity model and other deterministic formulations, more recent studies focus on the process of spatial movement, particularly on behavioral aspects of migration.<sup>50</sup>

Heide outlines the significance of migration models for population forecasts in his review article.<sup>51</sup> He sees the distance factor purely as a surrogate and finds both the gravity information of Zipf and the intervening opportunities model of Stouffer lacking in predictive power. He goes on to speculate that economic, social, and psychic factors (e.g., amenities) are much more important than distance per se.

Migration models in geography and elsewhere appear to be generally consistent with reality and apparently could be used for predictive purposes. From the policy makers point of view, however, questions related to how well the variables used in these models can be controlled are of major importance.

### 3. Forecasting and Predictive Models related to an Urban Context

Geographers have long shown a strong interest in location of activities in cities and between cities. As such there is a voluminous literature dealing with the analysis of urban phenomenon. This section discusses several sets of models dealing with various aspects of urban areas.

#### a. Urban Systems - The Location of Cities

While a detailed review of central place research is found elsewhere in this monograph, it is important to note that many of the models developed under the rubric of central place theory are, or could be reformulated into,

predictive models.

Initially much of this research was directed toward the description of structural characteristics of urban systems and was based on the pioneering work of Lösch and Christaller.<sup>52</sup> However, Berry and Barnum restated central place concepts in a system that would permit prediction of one or more variables in terms of the others.<sup>53</sup> This was done through the use of structural equations, related populations, density, central functions, and number of establishments through generalizations regarding the aggregate features of central place systems. In another article, Berry, Barnum and Tennant investigated the size and spacing of central places, their locational patterns, consumer travel behavior, systems of trade areas, and their interdependencies.<sup>54</sup> They then proceeded to estimate parameters for a set of linear regressions based on these variables which clearly have predictive value.

Morrill's predictive model of the spatial distribution of towns in Sweden is based upon an understanding of the process of urbanization and the contributing factors, including the explicit recognition of an element of uncertainty or indeterminacy in all human behavior.<sup>55</sup> In his model he integrates cumulative knowledge about central and non-central place activities, migration processes, and transportation to simulate and predict the spatial distribution of towns.

Brown has indicated that principles which prove satisfactory to the analysis of spatial diffusion processes may also serve to account for any type of movement in geographic space.<sup>56</sup> Hence he expects a coalescence

of approach between geographers who study the dynamic aspects of location of economic, urban, social, and political phenomena and those that emphasize diffusion. In a majority of his work to date, however, this expected coalescence has not appeared. Hudson, on the other hand, utilizes a diffusion process of expansion, competition and relocation and incorporates them into a dynamic model whose equilibrium solution predicts the central place pattern of settlements for Iowa.<sup>57</sup>

b. Urban Places - The Location of Activity

A large literature exists which focuses on both the aggregated and disaggregated analyses of the location of activities in cities. Contributions to this literature were made not only by geographers but other social scientists as well as researchers with engineering and operations research backgrounds. Varying degrees of theoretical elegance are used in the literature ranging in complexity from simple curve extrapolation and projections of past trends into the future to allocations, based, for instance, on the gravity principle to predictions based on parameters established by regression analysis, through those models that are based on an understanding of locational processes, and/or deductions from a theoretical framework.<sup>58</sup>

1. Models of City Growth and Development

Well known classic models of city structure include the concentric circle or zonal model, the sector or wedge, and the multiple nuclei formulation.<sup>59</sup> These are symbolic generalizations concerning city form and structure. The latter model was formulated by Chauncy Harris and Edward Ullman in 1945 by combining aspects of the other two.<sup>60</sup> More recent

formulations are less well known. These recent models have been constructed during the past decade usually in the context of transportation planning studies on a metropolitan wide scale.

Urban structure at the metropolitan level can be viewed as comprising both static aspects of form and dynamic functional relationships that exist among social, economic, and political components of cities. Realization that urban processes are interrelated phenomena and, as such, individually and collectively determine urban development has led to the building and use of comprehensive models. These complex models usually embody some form of spatial allocation mechanism that provides economic and/or demographic estimates for sub-areas of a metropolitan region.

This discussion is not addressed to any one model as such, but rather focuses on the current nature of comprehensive models of urban development, on the spatial aspects of the modeling of intra-urban phenomena and goes into some detail as to modeling strategies. While the general case and strategy discussion focuses on urban models, it is applicable to forecasting models in general.

The General Case: In general, urban development models have been designed for purposes of replicating the pattern of land-use in the urban area at some future time period. Toward this end, features of the existing patterns, past trends, known public or private institutional decisions, and other information are necessary as inputs. The model builder to some degree must specify the spatial and functional relationships among these elements. This may involve the utilization of existing concepts concerning the nature of the

relationships or they may be derived empirically.

Prediction may not be the intended purpose of the model. The model may be designed specifically for purpose of investigating the structural characteristics of urban activities, with interest focusing on the testing of its specified relationships (i.e., its validity). In either case, to be tested a model must be operationalized. Testing and utilization, except in the simplest of cases, therefore involved programming the model for computer solution.

The nature of a general urban development model is easily schematized. Figure 1 depicts the major features of the model building process. This simple diagram is by no means complete nor is it implied that the sequence is necessarily as shown. It is obvious that if sufficient theoretical concepts are available the relationships may themselves be specified as inputs. We have maintained simplicity as the diagram is intended to portray only the nature of the modeling process, thereby providing a perspective by which current research can be related to these endeavors.

Figure 1 depicts four steps that must be considered in any modeling effort. The collection of pertinent information about the metropolitan region is obviously necessary. Three sectors characterize this step; existing patterns in time  $t$ , the environment in time  $t$ , and known decisions occurring during a specified time interval that will influence locational decisions and therefore affect the patterns at a future point in time  $(t+1)$ . The arrows clearly indicate the direction of reasoning at this stage, but an example is useful. Consider the prospect of a new highway opening during the time

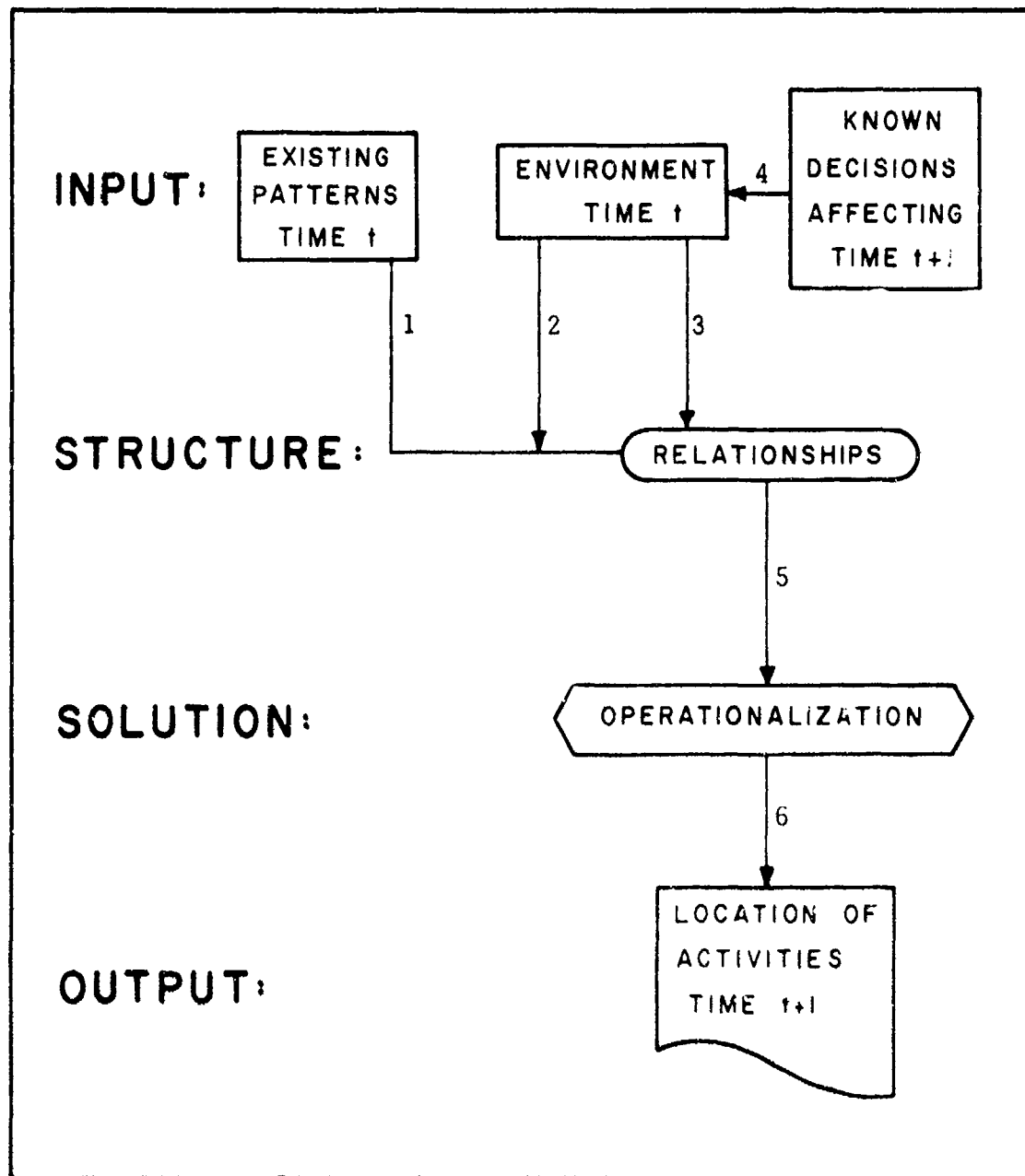


Figure 1: The Modeling Procedure



interval of interest. This would alter the pattern of accessibility (say to the Central Business District) within the urban region during the period of development and should be considered. Thus, while the network geometry of the transportation system may not be explicitly considered in the structural relationships (step 2), it is an environmental component of the system being modeled. This type of alteration in existing patterns is shown by arrow 2. Inputs indicated by arrow 3 are related directly to the structure, say a decision to alter the established norm between "population" and "area" devoted to public schools.

Inclusion of this "environment" in the diagram is quite useful in that it emphasizes the abstraction that takes place between the INPUT step and the STRUCTURE step. Not every variable in the urban system can be included in the structural relationships which will determine the location of activities in time  $t + 1$ . Arrows 2, 3, and 4 indicate both the need to specify the variables not endogenous to the system that may have locational impacts and, second, the manner in which these variables can enter the model.

The possibility of known relationships being specified as inputs was noted previously, but if such is not the case then an analysis of the data inputs will be necessary in order to determine them. Step 5 in the diagram emphasizes the disparity between specifying these relationships and our ability to operationalize them in a formal solution. For instance, it has been hypothesized that intra-urban migration of households is in part explained by the "action space" of the household head. However, it is one thing to say that maximum search radius ( $R$ ) is directly related to the size of one's

action space (S), age (A), income (I), number of family members (N), symbolically, and it is quite another to specify the proper equation. The problem

$$R = f(S, A, I, N)$$

is seen to be even more complex when it is realized that action space itself is a function of age, income, family, and several other attributes and variables.

Step 3, SOLUTION, says that each relationship must eventually be operationally defined, emphasizing the handicap placed on more operationally oriented model builders that does not burden the theoretician. The model user must get results (Step 4, OUTPUT) not pose research problems. The solution of the model will provide the location of activities at time  $t + 1$  as output. Conceptually, the modeling procedure relates that which is known about the location of activities in time  $t$  and the processes of change which influence the location of activities in time  $t + 1$ .

A majority of the data inputs to models of urban development are given in location specific form, most often in the form of being located in census tracts, origin-destination zones, or similar areal units. The structure of the model must specify the spatial aspects of the functional interrelatedness of urban phenomena.<sup>61</sup> In general urban geographers have not addressed themselves to the comprehensive interaction among activities important in the spatial allocation of activities within urban areas. This is not meant to imply that geographers have completely ignored urban models of the type considered here. Malm, Olsson, and Warneyd have contributed to this literature as have Morrill and Lakshmanan.<sup>62</sup> The Malm article discusses

a system oriented model that indicates those sites chosen for "development" over a given time interval.<sup>63</sup> Parcels to be classed as developed at the output stage are selected by sampling from a probability distribution based on a combination of defined space preferences. These include such features as distance from existing nuclei, local terrain and ground conditions, construction costs and political decisions. Morrill's research, on the other hand, is a process oriented model that is operationalized in a very simple manner such that its solution may be carried out by hand.

Neither of these studies can be considered on a level with the urban development models that have originated from other sources; namely, the major metropolitan research efforts of the past decade. But the work of urban geographers in the urban modeling field should not be dismissed so easily. While comprehensive development models generally have been superficially treated by geographers, specific subject oriented models have received considerable attention.<sup>64</sup> In many respects these models require the use of the same strategy as the urban development models. Because of their specific subject matter, however, it is frequently possible to develop stronger conceptual bases and to be more specific in the causal relationships which form the structure of the model. Such experience provides the geographer with the capability to critically evaluate more comprehensive models.

The Strategy of Urban Development Models: There are a number of general characteristics either explicitly or implicitly considered in all urban development models. Briefly, these are

- a) choice of areal units of observation
- b) growth (decline)

- c) spatial allocation mechanism (inter-sectoral relations)
- d) sequence of events (inter-functional relations)
- e) equilibrium assumptions,
- f) time interval, and
- g) quantitative input and output.

The choice of areal units is quite frequently dictated by data availability. Census tracts, or areal units of some uniform size provide the necessary spatial dimension. The mode of activity allocation is more often dependent on the conceptual framework than is the choice of an observational unit. The model builder is open to a wide range of choices at this point. Activities may be allocated directly or one activity type may be transformed into another using established norms such as numbers of people into square units of public roads or amount of land devoted to public services.

The strategy used in handling urban growth and decline is quite often overlooked. It is not uncommon to find that separate models are utilized for each case. Frequently, too, is the case where only population growth or economic expansion is considered; no provisions are made for a non-growth possibility. It is interesting to speculate concerning the changes that might ensue in the "inputs" and "structure" of urban development models if the objective were to simulate change without the possibility of growth brought on by population increase or economic expansion.<sup>65</sup>

The geographer should be expressly concerned with the manner of distribution of activities (competing land uses) among the areal units. The basis of this allocation mechanism may be a projection of past trends, empirically derived equations expressing hypothesized relationships, repeated matching of locational criteria with area characteristics, or a combination of

these and a host of other procedures. Since few of the actual relationships to be described are known in a causal sense the structure of models at this point diverge considerably.

The specification of the sequence of events, assumptions about equilibrium conditions, and temporal aspects are interrelated but separate problems to be considered. The urban development model must consider the totality of relevant activities and their interrelationships in time and space. Chapin makes the distinction between "primary" and "secondary" actions.<sup>66</sup> A decision to develop a new industrial park, airport, public building, or traffic artery in a specific sector of the city would be expected to initiate additional development, both public and private, in the adjacent areas. By controlling decisions on the primary level subsequent secondary actions of a given type can be encouraged in one or several areas to the exclusion of others.

The cumulative results of secondary actions over an extended time interval may influence the location of a primary action. Residential expansion demands public services, new roads for example; nonetheless, these can be strategically located in accordance with expressed development policies. On a day to day basis, however, locational decisions are influenced by the previous "primary activities". The inertial effect of all previous location decisions is also a well recognized feature.

Whatever the nature of this sequence, the researcher must consider that the specifications lead to some form of equilibrium or quasi-equilibrium. This fact is now always explicitly considered by the model builder. Once

the sequence of events has completed a cycle, the development becomes stable, and unless some further primary action is induced spatial structure becomes a constant. Adjustments in the pattern of activities are incorporated during the development sequence and no subsequent adjustments are considered. This feature of urban development models makes the choice of a time interval a very relevant question.

Generally, studies which have utilized models have had as their primary objective the projection of intensity of activity or land use by areal unit at some future point in time, and frequently this information is utilized to provide estimates of traffic flow or other linkages. This objective may be accomplished via a single step mechanism or a recursive solution. The recursive situation implicitly assumes that while growth creates form, form influences growth. It becomes relevant then to ask at what moment or after what length of time do previous decisions become influential for subsequent actions. What variations exist between activities with regard to this time interval? Should the cycle time of the model attempt to replicate these features, or may the time interval be based strictly on the availability of time sequenced data sources?

The above features of urban development models may be considered explicitly or implicitly in the model building process. Their existence raises important questions and points out several potential research areas. These include investigations of the exact nature of sequential events in the location of urban activities and the degree to which assumptions of equilibrating tendencies are valid. Likewise, the time lag for these conditions to become

effective should be considered so that appropriate time intervals might be suggested.

The final feature suggested as being a part of any urban growth model also relates to the data base. The data inputs and outputs for the model are of a quantitative nature when operationally defined. These include characteristics of the population, activities, areal units, as well as relative measures of location and other features which may be considered influential in the development mechanism. Availability of appropriate data will restrict the choice of variables open to the model builder. Frequently in practice the operationalization of a model results in a scaled down version of the original conceptualization. In effect, the nature of the data inputs will in part determine the form of the model's structural relationships which must be established.

Output variables may be graphically displayed (e.g., a population density map), printed for report generation, or stored for later use. Clearly, developing a comprehensive urban model requires the collection and manipulation of large amounts of data as well as difficult decisions as to which variables are to be included. Variables are suggested by the conceptual framework upon which the model is based, but often surrogate measures must be defined which often require the utilization of powerful mathematical and statistical techniques. For example, a complicated accessibility measure comprised of both time and distance variables may be required to relate each area to every other area in the study region.

The combination of choices concerning the features mentioned above comprise the modeling strategy. There are several articles addressing this aspect of modeling which will provide a reasonable background on the salient features of models of this nature.<sup>67</sup>

A comparison of models usually consists of describing the position of the individual models on the basic dimensions previously mentioned or on other relevant features. Harris contributes one such review while presenting an assessment of urban development models for metropolitan policy making.<sup>58</sup> More recently, a framework has been presented for the analysis of models where each model is distinguished by its basic characteristics.<sup>69</sup> These basic elements include the subject of the model, its function, the underlying theory on which the model is based, and the method(s) utilized for the solution.

A structural comparison of urban development models prepared by Lowry considers a market paradigm which serves as a point of departure for the analysis. The market clearing solution of the paradigm provides a useful basis for comparing the models in terms of their formal theoretical structure. Utilizing this conceptualization of the urban land market as a standard or norm provides for a coherent discussion of these models. Wilson, Lee, Horton and Hultquist, and King have also provided reviews of this literature.<sup>70</sup>

## 2. Urban Subsystem Models

In addition to the attempts to develop a model which encompasses the sum of the activities in urban areas many studies pursue the location of activities within a more narrow framework. While many of these models attempt to define structural relationships, rather than to predict or forecast



individual activity location, they are easily convertible into predictive models. The vastness of this set of literature precludes anything more than a very cursory view of the several subsystems which might be broached.

Residential location in cities has been an important subject of interest for many researchers interested in urban spatial structure. Wingo's classic work on the development of a theoretical model for identifying the components of the residential land use choice process in urban areas preceded much of the quantitative work in this area.<sup>71</sup> Muth's work on the spatial structure of housing in the cities has continued to play an important role in formulating econometric models of changes in the housing market.<sup>72</sup> Herbert and Stevens' optimization model, developed for the Penn-Jersey transportation study, is a classic example of the utilization of optimization models for determining the location of residences in urban areas.<sup>73</sup> An alternative optimization approach is offered by Schlager in his development of a recursive programming approach to the residential land development process.<sup>74</sup> The work of Chapin and associates in the simulation of residential development clearly falls within a predictive framework.<sup>75</sup> An application of markovian analysis to understanding change in the rental housing market is also an example of a methodology for predicting residential location.<sup>76</sup>

A separate line of research has been the interest in population densities in cities. In the main this work has been primarily devoted to the derivation of functions describing population distributions and important inputs to it. Typical in this respect is the work of Berry, Simmons and Tennant which follows that of Colin Clark and parallels the work of Newling.<sup>77</sup>

In some instances the comprehensive urban models discussed earlier treat household allocation as a separate submodel. A case in point is Crecine's time oriented metropolitan model (TOMM).<sup>78</sup> In the allocation of the residential population this model incorporates two kinds of costs, being added they are therefore considered without regard to mix. These expenses involve the site rent and the travel costs incurred by the household. The site rent as a function of the urban property market will normally vary with general accessibility and is in itself a spatially oriented variable. The travel cost can theoretically be considered as specific to the individual household and a function of its travel patterns. It has not been established that the characteristics of the travel pattern of a household or the travel cost co-vary with changes in site rent or relative location. Obviously the number of factors which enter into any substitution mechanism exceeds two.

Other factors considered in the household's locational decision as specified in TOMM include the condition of the buildings, quality of school facilities, public services, and the proportion of households of the type similar to the locating household. Taken together these many factors are considered as a bundle of goods and services which permit an area to be compared with other areas and matched with a household's requirements.

Prior to the Crecine model, Lowry also specified a separate residential location function in his "Model of Metropolis".<sup>79</sup> These residential sub-models are obviously directed explicitly toward prediction. In another vein Ellis' work on a residential location model focused on incorporating behavioral elements in an attempt to initiate residential choice patterns.<sup>80</sup>

Initial research into intra-urban commercial structure began with identification of the description of city retail patterns by several researchers including Rolph, Proudfoot and Ratcliff.<sup>81</sup> This research focus shifted to the investigation of spatial organization in internal hierarchies in urban commercial structure as an extension of central place theory, particularly the works of Garrison, et. al., Berry, and Carol.<sup>82</sup> Although there appears to be a decline in research related specifically to commercial activities with cities, a refocus on the process of retail location is apparent in more recent literature. The work of Berry, Simmons, Lowry and Crecine are indicative of this approach.<sup>83</sup>

Even more recently there seems to be a diversion by geographers which focuses on the consumer and his behavior rather than the location of opportunities. The research of Horton and Reynolds, Rushton, and others are indicative of this trend.<sup>84</sup>

Very little work has been completed in the area of identifying predictive models of industrial location within cities. Geographic literature is basically descriptive and taxonomic in this area.<sup>85</sup> However, Putnam's model of intra-urban industrial location clearly develops an operational capability to predict location in cities.<sup>86</sup> Moses and Williamson's study of the location of economic activity in cities was also an attempt to construct a predictive model of industrial location.<sup>87</sup> Although little has been done this line of research would appear extremely fruitful for the geographer.

With respect to the location of public facilities in cities, the paucity of literature is evident. Teltz's work on a theory of urban public facility

location was a land mark and perhaps will provide continuing impetus for evaluation of public sector decisions.<sup>88</sup> Czamanski at approximately the same time was developing a two stage model for predicting and identifying the impact of public investment on urban land values.<sup>89</sup> Since the location of public activities could be an overt policy decision, it is surprising that demands for such research have not been stronger. As the importance is recognized, development in this area will most likely be based on clearly defined optimization models which best allocate the public resources. Therefore, it would follow that predictive models will not be utilized extensively in this area.

#### 4. Normative Economic Models

Many of the conceptual and theoretical models developed by economists for application in regional analysis have become an inherent part of the geographic literature dealing with regional and economic analysis. While a part of this literature has been discussed in a previous section (location of cities), such normative approaches to the location of economic activities as interregional linear programming, interregional input-output analysis, supply area analysis, and market area analysis have not been addressed. In the main, these models are utilized to indicate a variety of spatial relationships and interregional flows which are inherently of interest to the geographer.

If we conceive of these models as providing some of the foundation for understanding commodity flow and location behavior, all of them can be applied in a predictive-forecasting context. Interregional input-output analysis, for example, can use exogenously derived output vectors for some industries and

predict interregional flows of commodities on the basis of that growth.<sup>90</sup> The same holds true for interregional linear programming with the difference being that an optimal flow solution can be determined.<sup>91</sup> In the case of market and supply area analysis, the forecasts of industrial locational behavior is possible as are forecasts of the distribution of supply levels emanating from various locations.

These models suffer from rigidity of assumptions upon which they are constructed. In general, they are of more value from a theoretical point of view than from an applied point of view, although interregional linear programming and interregional input-output analyses can be extremely useful in a policy context. When dealing with the nuances of the real world, variation from optimal and normative solutions can be expected because industries, consumers, regulating agencies, and the like neither act in a strict economic fashion nor under the rules of perfect information. Therefore, the application of these models in a forecasting sense must be viewed with some caution.

## 5. Transportation Models

Under the general rubric of transportation models, a relatively clear separation of objectives and modeling strategies has been used in evaluating large-scale transportation networks (intercity) and intra-urban transportation systems.

### a. Network System Models

While networks have been analyzed in a variety of contexts, the principle interest here is prediction of network change (growth-decline) and flows. Geographers have been interested in both of these aspects. The

evaluation of network growth and development is attributable primarily to Taaffe, et. al., Haggett, Gauthier, Garrison, Werner, Boyce, and Black.<sup>92</sup> Research in this field has been pursued in a variety of ways, ranging from a relatively descriptive approach, to network development, through statistical models attempting to predict temporal aspects of individual link development. At the present time the discipline is witnessing a leveling off of this activity although a majority of the forecasting models need a great deal of further effort before a satisfactory ability to predict network development is accomplished.

The network flow literature, while dominated by other disciplines, particularly communication and electronic engineering, has been utilized and applied in particular contexts in geography. Once again the utilization of optimization methods such as linear programming abound. Obviously the utilization of gravity models in order to evaluate in a more general sense commodity flows has also been used quite extensively. Network flow analysis generally takes on a policy element in the sense that concern focuses on identification of optimal flows and optimal networks for given situations. When viewed in terms of their ability to provide policy alternatives of quasi-optimal nature with near certainty of adoption, the models do, in a sense, predict network configuration and flow.

b. Intra-Urban Traffic Models

Since transportation and movement in cities play an important role in their morphology and the distribution of activities within them, travel behavior and traffic flows in cities have long been of interest to the geographer.

Geographic concern with the modeling of travel behavior in cities might be traced to Garrison, et. al., and their research on highway development and geographic change. The growth in federal expenditures related to transportation planning and subsequent concern with forecasting travel behavior in cities provided geographers with increasing amounts of relevant data. Currently used in the forecasting procedure are:

- (1) trip generation models,
- (2) modal split models,
- (3) trip distribution models,
- (4) assignment models (trips to the network links).

With the exception of models of modal split, geographers have participated in the development of modeling structure and the application of these kinds of models in a variety of contexts.<sup>93</sup> While emphasis varies, there is evidence of a movement away from furthering the development of these models in particular areas and increasing interest in evaluations of individual movement patterns. Contributions by Murdie, Rushton, Hurst, Chapin, Wheeler, and Horton and Reynolds are an indication that a serious attempt to understand individual travel behavior is underway.<sup>94</sup> While the aggregative methods of travel forecasting in cities are useful in an operational sense, they lack a well-defined theoretical structure.<sup>95</sup> A growing interest in behavioral models is indicative of the current thinking that the urban travel behavior process is not separable into different sets of decisions, but rather travel demand, model choice, patterns of movement, and route choice are so highly inter-related as to logically require a composite within-city movement model.

### New Directions in Predictive-Forecasting Models

The previous sections have outlined numerous lines of research which have and can lend themselves to the development of predictive and forecasting models. But what of the future? What changes can be foreseen and what changes and/or new directions in geographic research focusing on predictive models should take place?

There can be little doubt that "applied" geography has not been the rallying cry of a large number of geographers. However, there appears to be a subtle change taking place within the discipline. The recent development of task force formulation in numerous problem areas by the Association of American Geographers is only one outward manifestation of the change in attitude and perception of the role of geography as an applied and policy science.<sup>96</sup>

The increased emphasis on applied research related to relevant issues by federal funding agencies if continued will certainly cause irreparable damage to the scientific community in general and to geography specifically. However, in the short run it signals the need for currently usable products to solve the crushing problems facing our nation. An equitable distribution of basic and applied research is a necessity and recent history has shown time and time again that these two ill defined research foci overlap and are highly interrelated.

The output of pure or basic research in geography has for the most part been meager. On the other hand applied geographic research has also been somewhat limited. Thus, a majority of the research output has fallen



somewhere between these two more productive ends. The current need for predictive and/or forecasting models is greater than ever before. The extremely high cost of capital investment and the time cost related to institutional change are such that wrong decisions often result in a debilitating round of effects which complicate and further the multifarious problems which the decisions were designed to alleviate.

Geographers for the most part have stayed on the sidelines in many problem areas where their expertise and abilities could have been extremely useful. Their concern with a broad range of processes make them a prominent member of the social science community. Their ability to communicate with many other disciplines ranging from astronomy to zoology provides a strong foundation for their participation in interdisciplinary team research efforts of the kind necessary to adequately define and assess problem solutions. So it is that geographers should and will engage in a larger proportion of vital applied research activities. While construction, evaluation, and testing of predictive/forecasting models are only one aspect of their activities it is a prominent one.

The current state of prediction and forecasting capability in the social and planning sciences are for the most part in their infancy and as such important research remains to be accomplished. With the advent of quantitative sophistication and rigor, geographers are beginning to acquire the needed credentials to adequately pursue this line of research activity. As indicated previously a set of models applicable to a variety of processes are already available in the literature. Some of these are extremely useful

in their present form; others need a greatly increased amount of continued and productive research. Basic research being carried out today will help provide the basis for restructuring current and satisfactory models. In other instances new methods of data collection will provide the means for constructing more comprehensive models and/or more suitable variables for surrogates for particular problems.<sup>97</sup>

Several issues are clear. Limited resources will require accurate forecasting of the impact of alternative policies in a variety of contexts. Our society cannot be dominated by forced solutions and controls and thus, a more satisfactory utilization of indirect controls are necessary. Because of scarce resources objectives must be clearly defined and goals which can be translated into quantitative measures by which programs can be measured must, and should, become a reality.

The above allegations have direct consequences for the nature of predictive and forecasting models. Models in their present form allow too much latitude in predicting configurations of various phenomenon. As goals and objectives become more rigorously defined, the nature of these models change and the answers sought through their application also change. Models based on past trends have utility, but if their purpose is to predict change in order that it may be accommodated, the assumption is that the past was good. Further models will be most useful if they are constructed in such a manner so as to provide information that would allow the "optimal" utilization of limited resources in order to achieve a maximum number of objectives or come closest to reaching our

ultimate goals.<sup>98</sup> These kinds of models in general might be labeled goal oriented impact evaluation models. That is, in attempting to achieve certain goals it is necessary that certain actions be carried out. These actions must in turn be evaluated as to their impact on the particular system being changed to conform to the aspirations of particular decision makers based on the needs of a particular community. For example, earlier models concerning urban growth and development were discussed. These models, in general, are currently used to forecast the distribution of activities in cities. If a priori decisions as to the form and morphology of a particular metropolitan area or the location of activities within it are made, we would want these kinds of models to evaluate the impact of particular capital investment decisions on "coercing" the private sector and the city's inhabitants into making decisions which would achieve the stated a priori goals. In an urban context, and in fact in many other spatial planning contexts, it is often inexpedient from a political point of view to define a goal in such an explicit manner so as to provide sufficient guidelines for current and future actions. Irregardless of how difficult this type of goal formulation is, it must be accomplished or many more of our current problems will remain with us for some time to come.

In the reformulation of current models and the construction of new models greater emphasis must be placed on the utilization of control variables. That is, if at all possible model variables should include to the greatest extent possible variables over which the public sector has some control. King has noted that:

"given that society now seems willing to consider questions of spatial and environmental planning much more seriously than in the past, then there exists an opportunity to move purposefully in some of our work towards topics in decision theory, adaptive processes, and control theory. Decision theory has appeared in our literature in the context of analyzing the behavior of actors in space. What I have in mind now is the casting of certain spatial planning problems in the framework of decision theory. The paper by Hutchinson is in accord with this proposal. Adaptive processes have been characterized by Murphy in terms of four fundamental aspects: (1) there is a set of alternatives about the problems of the present, (2) there is some objective which is to be optimized by the selection of a subset of possible view; historical information and experience is critical on the formulation of this optimum current decision, (3) a choice mechanism is necessary to determine which subset of views is optimal to society, and (4) a police system is necessary to impose the optimal choice on the holders of other subsets of ideas. These notions seem quite consistent with the structuring of policy and spatial planning. Control theory relates more to dynamical systems which are characterized by input/output relations. The problem is how to manipulate the inputs so as to force the system to produce the pre-assigned output."<sup>99</sup>

King goes on to note several illustrations of this kind of approach in relation to alternative problems.

King's statement may be characterized as providing a three step analysis phase for any applied problem. That is, alternative solutions must be defined, a decision must be made concerning alternative ways of bringing about a solution, and some form of control must be utilized in order to impose the solution upon the society or community. While at first glance this may appear quite drastic in the kind of political system in which we currently operate, it is in fact the way the system now operates. However, for the most part these types of decisions and the action associated with them take place and are masked or made to appear only implicit in

order to soften the imposition on individuals and firms. However, this softening and non-explicit statement of objectives often leads to incomplete or inefficient solutions.

Gates and Anvari have written on the development of a control model in a specific context, regional water management.<sup>100</sup> The purpose of the model is to provide decision makers with an evaluation of the cost-effectiveness of specific water management policies. The cost of a specific policy  $x(t)$  is given as:

$$\sum_{k=1}^n \int_0^T \lambda_k \left| x_k(t) \right|^p dt$$

where:

$\lambda_k$  = constant,

$p$  = is a numerical value greater than 1; it indicates that the cost of a policy is exponentially related to its size,

$x_k(t)$  = an n-vector of substrategies  $x_1(t), \dots, x_n(t)$  are functions of time.

The object of the model is to define the minimum cost strategy given that there is a defined initial state and a desired end state. Gates and Anvari go on to develop a theorem which provides the basis for selecting the minimal cost strategy to accomplish the desired end state. While their model is extremely abstract and does not address several important definitional problems, it illustrates the kind of models which are necessary to achieve objectives in a scarce resource context. In addition the model explicitly defines the management control function and it becomes an inherent part of the total model. Their model represents one extreme, that is, the

inclusion of management as an element of the model, however, it is the kind of model which is extremely appropriate for their problem. In fact, many applied models would do well to include a management component, thus, allowing for explicit definition of the implications of management decisions.

While Gates and Anvari are addressing a physical system, more difficult problems accrue to the modeler focusing on socio-economic systems. Explicit control of individuals and firms is an anathema to many nations in varying degrees. In fact very few controls exist in the United States which can aid institutions burdened with spatial planning activities. Therefore, while optimization models, or more properly sub-optimization models, are particularly useful in evaluating the proper use of scarce resources to obtain specific goals, the impact of a policy on individual and firm behavior can best be evaluated by different forms of models.

The increasing concern in geography with behaviorally oriented models, previously discussed in this monograph, will provide important benefits for addressing the problem of individual and firm response to alternative policies. Using the urban example, urban planners may determine the most appropriate solution to the problem of activity location with respect to a variety of criteria and proceed with capital investments concerning restructuring of the transportation system and assume that individual and firm response will be such that particular kinds of activities will respond to the transportation in the system configuration in a certain manner, thus, achieving the final result by indirect means. However, given the current state of our knowledge of travel behavior and economic activity

response to transportation, only gross generalizations might be stated. It is clear that individual inhabitants of the city cannot in and of themselves force changes in the location of activities or force changes in the location and configuration of transportation systems and modes. In the main, individuals and firms are responsive to changes in the environment rather than the cause. New developers purchase land in close proximity to planned expressway development and reap profits which in reality are generated by public funds used to build expressways. In effect, it is the contractor's response. Therefore, a thorough understanding of the response to various policies and public improvement programs is a necessity. Research that geographers are undertaking related to private and public sector and individual response given actual or perceived changes in the environment is a particularly useful line of research to develop models of this kind. Needless to say, a great deal of additional work must be accomplished in this area.

In summary, it should be noted that geographers can play a large role in helping to solve diverse sets of problems. Spatial planning activities will require restructuring and new development of predictive and forecasting models which help to utilize scarce resources in a more efficient manner. Research related to the development of optimization and sub-optimization models, the integration of decision and control theory into the modeling efforts, and research related to clearly identifying the response to particular actions by various sectors must be established as priority research activities by geographers.

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## CHAPTER VIII

### QUANTITATIVE SPATIAL RESEARCH IN GEOMORPHOLOGY

#### Introduction

Geography shares the study of land surface form with several fields, but most notably with geology. The area of overlap is most commonly known as "geomorphology", although various other terms have been used to discriminate the geographer's special interest in the land. The chief differences in approach that can be recognized contrast three broad groups of undertakings: descriptive, historical (genetic), and process-oriented approaches. The contrasts between the geographer's and the geologists' concern with the land surface need not concern us here except insofar as these differing viewpoints have affected the development of quantitative geomorphology in the respective fields.

So far as geography is concerned, the principal goal is taken to be the understanding of the landscape and its arrangement on the surface of the earth. In pursuit of this goal, generalizations and laws of a specifically spatial nature are sought. These generalizations may be considered in a spatially continuous context, such as distance relationships, or they may be spatially discrete, as in the differentiation of areas.

The history of development of landscapes, or the processes by which landscape develop, are considered to be relevant only insofar as they contribute eventually to an appreciation of arrangement or spatial order in surface form, or in variations in this order from place to

place. This is not to say that a total understanding of the landscape does not encompass a knowledge of its history and nature (or processes) of development. Quite the contrary. But if any division of labor is meaningful in science, it seems most appropriate that the geographer focus his attention upon spatial order (or the relative lack of it), and not dissipate his efforts upon earth history or upon detailed analyses of processes in an aspatial context. Lest there be misunderstanding, it should be re-emphasized that this is a statement of broad goals and paths, and should not be taken as a discouragement of individual studies which may appear to be purely historical-genetic or process oriented.

The preceding statement sets the path this review will take. There have been many, many studies in geomorphology that have used numbers. The focus here will be upon those efforts that have either explicitly or implicitly dealt with the understanding of the spatial order of land surface form. The development of this area of inquiry has proceeded from simple quantification or measurement of surface features through the statistical analyses of data to the derivation of generalizations and theory by the use of mathematics. We shall attempt to follow this progress by the device of viewing "threads of inquiry" as they have developed in the field. For reasons that are not entirely independent of what has transpired, and for purposes of efficiency, the present study will deal largely with the post- World War II period, and particularly with the English language literature. There was a great deal of early

work in the description of land surface form, particularly in the European literature. This has been summarized by Neuenschwander,<sup>113</sup> Hook,<sup>68</sup> and Young<sup>183</sup> and need not be pursued here.

Within this framework, Horton's 1945 article serves as a seminal study,<sup>70</sup> not that the landscape was not described quantitatively in previous works, as discussed in the three works cited above, but because this article focussed the attention of geomorphologists upon the necessity for considering complexity of landscape form rather than individual elements (cf. Scheidegger<sup>135</sup>) at the same time when it was becoming increasingly obvious that inductive studies to examine and back up broad qualitative interpretations of landscape development were sorely lacking, and at a time when large amounts of data and large numbers of studies dealing with individual landscape elements were becoming available from the work of the 1930's. Much of this latter work was occasioned by recognition of the fact that the environment was fragile and resources were limited, and were capable of supporting population adequately only if studied intensively. As a consequence, a virtual explosion in information became available in such necessary ancillary areas as soil erosion research and accumulation of hydrologic data. Although much of Horton's work was original with him, a great deal was being done on a similar basis simultaneously elsewhere. In this sense, Horton's "Erosional Development of Streams and Drainage

Basins" becomes a valuable summarization of the state of knowledge and an interpretation of how this knowledge could be organized, in the same sense that Davis' Geographical Cycle provided such a service and the impetus to further work more than a half century earlier.

The general dissatisfaction with purely qualitative studies and the desire for more precise, objective criteria on which to base interpretations led to work which proceeded along several paths, some of them quite independent of those that stemmed directly from Horton's research. It was as though there was an all-prevading awareness of some of the shortcomings of the work of the preceding decades, and this led to many independent attempts to do something about the situation. Several threads of inquiry developed, some of them as deliberately structured schools of thought, and some that may be grouped together on philosophical grounds in that they served similar functions or utilized similar data or techniques. It is difficult or impossible to categorize most studies as falling within one or another thread of inquiry to the exclusion of all others, just as it is frequently impossible to distinguish deductive from inferential efforts. In point of fact, deduction and inference are most commonly used in conjunction within the same investigation.

The threads of inquiry that may be recognized in quantitative spatial geomorphology may be grouped into three broad categories. These are not mutually exclusive classes, for many studies may be regarded meaningful as falling into two or more groups, as will be demonstrated later. For

the purposes of the present discussion, however, the division is a useful one, and includes:

1. Description of land surface form, including empirical explanations of the relationships between the elements of the surface slope envelope.
2. Fluvial morphology, including three main threads:
  - a. Stream flow explanation
  - b. Development of drainage nets and related topological considerations
  - c. Channel geometry -- valley geometry -- stream profile
3. Systems of landscape explanation, including slope development.

Other recent reviews of trends in land form geography and geomorphology have taken different tacks in considering the organization of the history of the field since World War II. Dury's<sup>41</sup> Perspectives on Geomorphic Processes is designed to fill the gap between research and texts as a supplement for college students and teachers. It is a succinct document that covers much of what has been here labeled "systems of landscape explanation". Dury is particularly useful for a brief exposition of what has transpired in "The Quantitative Revolution", most of which for reasons that will be discussed later, was concerned with fluvial geomorphology.

Zakrzewska<sup>184</sup> in her "Trends and Methods in Land Form Geography" groups studies into three categories:

1. Descriptive analysis of present land form
2. Functional analysis of land form, which includes the functional relationships of topography to other physical and cultural phenomena

3. Analytical studies of the covariation of individual land form elements. Many of the studies considered by Zakrzewska are included in the "Description of Land Surface Form" thread of inquiry.

Chorley<sup>24</sup> follows quite a different approach in viewing studies as models that may be grouped or classified into three kinds of systems:

1. Natural analogue systems, which may be translated in time and space
2. Physical systems, in which the landscape is dissected into integral parts
3. General systems, a broad conception in which phenomena are structured into a complete system from the outset, providing an "organization and operation of the system as a whole or as linked components rather than in detailed study of individual system elements".

The present review will concern itself chiefly with the description of land surface form, the first thread of inquiry that was recognized above. Much of what will be discussed is the result of application of physical laws to an understanding of surface form, some of the material derives from deduction from mathematical relationships among elements of the surface slope envelope, a great deal of it had no deeper purpose in the mind of the investigator than simply an objective comparison of different areas on the earth's surface.

It is within the "Quantitative Description of Landform" thread of inquiry that most of the material of interest to military Terrain Analysis has been developed. Indeed, in a sense the quantitative description of landform might be characterized as the civilian equivalent of terrain analysis. Rather few military studies are cited in the present review, for most of these works have not been available to a wide spectrum of non-military earth scientists, and hence were not involved in the development of the thread of inquiry through time. To set the stage for further discussion and to establish commonalities between the civil and military threads of inquiry, it is useful to provide a framework for viewing the form of the land surface.

#### The Continuous Surface Slope Envelope

The land surface may be conceived of as a continuous mathematical surface. In this sense, it consists of a continuous envelope of slopes or sloping forms. Divisions of this surface are most frequently arbitrary, in as much as individual forms, such as volcanic cones, sand dunes, and similar isolated hills are the exception rather than the rule. Consequently, one would speak of "landform" or "land form", in the manner of Hammond,<sup>60</sup> rather than of "landforms". Stream-dissected landscapes may be divided into stream basins, but not all slopes are easily assigned to one basin or another, particularly in regions of low relief. In any event, although stream-dissected landscapes are certainly most prevalent on the earth's surface, considerable land area is covered by young glacial plains, karst surfaces, sand dunes, and relatively fresh volcanic landscapes. These



surfaces are not stream-dissected, and therefore do not lend themselves to division into basins. Any universal system of measuring landform must take this fact into account.

Stream networks are integrated and consequently are orderly. Small tributaries flow into larger ones and eventually into master streams and thence into the ocean (or similar impoundment). The hierarchy of channels of various sizes may be classified according to their place in the system, or their stream order. Geomorphologically, the principal task of the stream system is the removal of surplus water from the land surface. In so proceeding, streams also carry sediment and denude the landscape. A great deal has been learned about this process simply by manipulating the topology of the stream network, as Horton and Strahler<sup>154</sup> and numerous other investigators have demonstrated. The slopes of a stream-dissected landscape feed the stream network in an orderly fashion, they are integrated with the channels and form part of the system of the basin. By contrast, the non-stream-dissected landscapes present a jumble of slope faces. Their surfaces are not the consequence of fluvial erosion, which involves the weathering and removal of sediment by running water and related mass-movement, but are due to other causes. Consequently, they are not integrated in an orderly hierarchy. It is more difficult to conceive of these surfaces as systems and to treat them in the same conceptual framework that has been applied to stream-dissected landscapes. Largely for this reason, the non-integrated land surfaces for the most part

have been ignored by the quantitative revolution in geomorphology, where most of the progress has been in the area of fluvial morphology. Fluvial landscapes present surfaces that are more easily treated topologically.

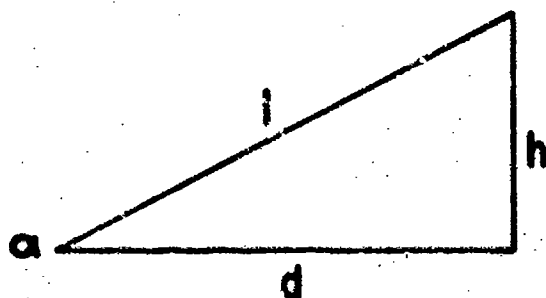
Topological considerations reveal a great deal about the spatial order of surface form. They may not deal directly with or contribute to an understanding of the physical mechanisms that carry out the denudation process, however. Those investigations that directly relate spatial order with the discharge of streams, stream loads, and slope development processes bear more simply on the understanding of the landscape in a broader sense. Most discussion on this point is deferred to the thread of inquiry dealing with fluvial morphology.

Process laws, as discussed by Bergmann,<sup>7</sup> are more likely to prove efficient in the construction of a theoretic structure that explains spatial order in the landscape than are cross-sectional laws. The latter may often be equated with simple empirical relationships that are limited in their application in time and space. Because of this limitation, they are not universal in nature. However, universal laws may develop from numerous cross-sectional laws if a research strategy is carefully plotted. It is not yet clear the extent to which cross-sectional laws and topological generalizations are essentially similar in their nature. One of the great values of process laws is that other laws may be deduced from them. This may not prove to be the case with topological generalizations, for as Bergmann points out,

"There is a hard core of cross-sectional laws, including those of geometry, that cannot be deduced from or in any other sense be 'reduced' to process laws. All-important as they are, process laws are therefore not literally all of science, not even in the ideal limit."

Thus, topological generalizations may prove useful as definitions of spatial order, even where they may not lead to the development of other laws.

All measurements of land surface form relate back to what may be called the fundamental landform triangle (Figure 1). Measurements of land surface form, or morphometric factors or variables, are all derivations of two basic dimensional and one dimensionless measure: the vertical dimension  $h$ , or relief; the horizontal dimension  $d$ , distance or length; and the dimensionless slope angle  $\alpha$ .



(FIGURE 1)

There are, of course, two length or distance dimensions. The one illustrated in Figure 1 shows the length across the trend of the slope. A second length measure can be visualized as being the width of the slope, and running along the contour of the land surface normal to "d". This measure is also dimensional in character, and may be designated "w". Within a stream basin,  $l$  and  $w$  may be synonymous, or indis-

tinguishable from one another in many circumstances. Either or both may provide a suitable operationalization of the concept of "length of overland flow", or the flow line that surplus rainwater would follow across a slope face before entering a channel. Both are necessary, however, in order to provide the third dimension in a conceptualization of landform as possessing length, breadth, and height.

On measurements derived from maps,  $d$  (or  $d'$  in the case of  $w$ ) is the distance or length measure employed. For short measurements, or in relatively flat terrain,  $l$  is virtually equivalent to  $d$  or  $d'$ . Even in quite rolling terrain, the difference between  $d$  and  $l$  falls within the range or limit of error of measurement from maps. Only the most precise field surveys for special purposes, as in rates of change of hillside slopes, or stream channel gradients, demand a specification of whether  $d$  or  $l$  is intended.

Slope profile considerations involve what is essentially a series of short  $d$  measurements, or more practically, short segments of  $h$  to  $d$  ratios. That slope inclination at a point may be considered as equivalent to this ratio has been placed on a firm mathematical-theoretic foundation by Strahler<sup>13</sup> by his use of the theorem of minimal values.

The vertical dimension or relief variable  $h$  is of primary importance. It is dependent upon the amount of uplift above the ultimate base level, conditioned by a distance relationship which is unknown, but probably related to a curve similar to that of many longitudinal stream profiles. The distance relationships from local base levels (stream valleys) and

from the sea when considered graphically may possess curves similar in form, but different in values of slope or intercept. The dimension  $h$  is critical because it is the source of potential energy for development of the system of landform, and thus for the denudation of the landscape.

The three dimensional and one dimensionless variables described above provide for the description of upland landform in what might be thought of as a three-dimensional topology. Stream channels, whether occurring in flat lowlands or on sloping upland forms, are frequently described in what may be termed a two-dimensional topology which lacks the dimension of relief, or vertical aspect. Applications of graph theory are examples. The development of a three-dimensional topology, describing the entire surface slope envelope, has long been deemed desirable, but has not been forthcoming. Morphometric variables approach different aspects of such a topology, but none define the surface area of landform. The various measures that have been employed may be thought of as extracting one or another element from the three-dimensional topology. Such extractions may serve particular purposes very well as surrogates, but it is highly desirable that some formulations which will define the actual surfaces of the land be developed. Only then will we know what the true yields of water and sediment are from the land surface on a density or "per unit of area" basis.

The Quantitative Description of Landform Thread of Inquiry

Horton's 1945 study was related to his interests in hydrology. Consequently, it dealt with stream-dissected terrain, and his techniques and ideas were not applicable to all types of land surfaces. At the same time, it must be remembered that disordered landscapes also serve as drainage basins in a hydrologic sense, even though surface flow of water is not involved and subsurface flow is complicated, leading to poor definition of the boundaries of drainage basins. It was to the quantitative study of fluvial systems that Horton's work provided the strongest impetus (Table 1). Not all of the early post-war efforts to quantify the landscape were devoted to the same ends that Horton envisaged, but his work undoubtedly produced an impact on these studies that might not have been fully appreciated at the time. In any event, means and ends become confused in the development of a research paradigm, and in the ensuing two decades it becomes increasingly difficult to segregate studies that may or may not be related back to the influence of Horton.

Much of the early work in quantification of landform was quite defensive in tone. The application of statistical methods to geomorphology and landform geography was a drastic departure from the techniques that had been pursued previously, and a great deal of rationalization and proselytizing was necessary before the methodology became acceptable on a broad basis. The early years saw much rather pure description, including the development of quantitative, non-genetic techniques of

classifying land surface form. There ensued considerable discussion as to why such description was necessary or useful. The answers varied according to the field in which the study was performed. In geography, it became increasingly clear that the involved development of denudation chronologies did not serve the purposes of economic or social investigations. Although geologists were becoming increasingly process-oriented in their geomorphologic work, rather than pursuing the development of the earth history or genesis of particular places, this did not necessarily entail the use of quantitative analysis. The fruits of the early years of labor were not harvested until much later, and, inevitably, many of the early plantings turned out to be barren.

In the 1950's, attention was turned increasingly to the relationships between elements of the land surface, rather than to the quantification of the surface for its own end. By the early 1960's the thread of inquiry was focussing increasingly on fluvial erosion and development. The generalizations that were developed were not universally applicable to the earth's surface, but were restricted to drainage networks. Only in studies of terrain for military purposes are attempts made to conform to some of the rationalizations that characterized the reasoning behind many of the pioneer efforts in quantifying land surface form. There may be good reasons for this development, for rather little research in the late 1960's has depended upon objective, comparable information about land surface form except in the military. Perhaps with increasing attention to the environment in

the next decade, there will be a resurgence of interest in functional relationships between landform and social-economic variables of the sort discussed by Zakrzewska.

The middle and late 1960's saw an increasing degree of attention given to purely topological work, but this was almost exclusively devoted to fluvial topography. This focus may be in recognition of the overwhelming importance of such landscapes, or it may be due to the ease of producing generalizations about integrated topography compared to the disordered land surfaces. Many of the generalizations produced for fluvial topography are also applicable elsewhere for hydrologic purposes, particularly when these derive from a study of the interface of the earth and the atmosphere.

The preceding brief review of the thread of inquiry into the quantitative description of landform sets the stage for a more leisurely examination of some of the ideas that were forthcoming in its development.

Following Horton's 1945 paper, there was a lapse of about five years time before any explicitly spatial quantitative studies dealing with the form of the land appeared in print, with the exception of some hydrologically-oriented work. Langbein's<sup>85</sup> 1947 paper on the Topographic Characteristics of Drainage Basins is a representative and self-descriptive example. In 1950 and 1951, a scattering of papers appeared, and two distinct schools of thought emerged. A more precise means of defining slope, relief, and extent of dissection in drift topographies so that different ages of glaciation could be objectively mapped was the goal of Ruhe's<sup>131</sup> pioneer efforts in non-integrated land surfaces. Although



the techniques were crude by later standards, utilizing highway profiles which rarely define perfectly orthogonal slopes, they were effective, and the papers have been cited countless times since they were first published. It is worth noting that the principal contribution of the first paper lies not so much in its differentiation of drift topographies as in the implications of their varying degree of development or dissection by fluvial agents. Some of the points raised by Ruhe in this work are still the subject of controversy and undoubtedly will not be resolved until a great deal more is known about the development of drainage nets and their implications in systems of landscape explanation. Very few other studies have dealt with non-integrated glaciated terrain, Reed, Galvin, and Miller's<sup>129</sup> 1962 paper on drumlin geometry and some of Salisbury's work (to be discussed later) being the principal exceptions.

Calef<sup>16</sup> published a paper in 1950 which was part of a series of four studies dealing with relative relief and average slope in Illinois. In a very real sense, Calef and his colleagues were engaged in an "anti-quantitative" quest. The techniques employed derived from pre-World War II methods used by Raisz and Henry<sup>126</sup> in New England, Smith<sup>147</sup> in Ohio, and Trewartha and Smith<sup>153</sup> in the Driftless Area. The subjectivity involved in selecting class intervals, choosing uniform regions for delimiting slope, and other pitfalls are well illustrated in a state in which such methods would be severely tested. Illinois consists of a

flat upland cut by two or three major stream trenches. The "average slope" as depicted by these techniques is meaningless, for it is a moderate slope that is rare in nature, most slope faces occurring on relatively flat uplands or steeply sloping valley walls. Although this pitfall can be eliminated by other methods of measuring slope angles (see Strahler's and Salisbury's techniques, discussed below), the collection of studies still serve a useful purpose in that they encourage caution in the choice of statistical measures and statistical units. Where Ruhe attempted to interpret the surface slope envelope from highway profiles, which can be considered to be traverses across the form of the land, Calef and his colleagues employed the notion of the uniform slope region as utilized by Raisz and Henry. Also considered was the use of grid squares, or quadrats, in the manner of Smith. Both these techniques involve areal units, and to a considerable degree aggregate or generalize the data. In fact, one of the principal problems relates to the depiction of the results of the analysis on maps, and therefore is not independent of cartographic considerations of scale and generalization. At the same time, the technique must also bear upon the difficulties of measuring the land surface in such a manner that areal properties are considered, and spatial relationships maintained. Ruhe's traverses, by contrast, are a sample of the land surface, and because the sample was not selected by statistical means, but rather by the expediency of highway locations,

it is not amenable to predictions of values for the entire surface area on a probability basis. Although Ruhe made an attempt to select only those highway profiles that crossed the grain of the landscape, rather than slicing it on the bias, there is no assurance that his traverses are representative of the drift topographies portrayed.

One of the schools of thought that emerged in this early period had its foundation in geography, and from all outward appearances derives very little from Horton's ideas. The other evolved in geology, and has as its foundation many of the ideas advanced by Horton, applying them to drainage basins and slope faces within a systems format. This latter school of thought includes the group who worked at Columbia University with Arthur Strahler.

The geographical school emerged from the dissatisfaction of many geographers with the classical "explanatory description" geomorphology that dominated the field before 1950. Richard J. Russell<sup>132</sup> enunciated the first call for reform in his 1948 Presidential Address to the Association of American Geographers in which he called for a geomorphology better suited to the needs of this field, i.e., one that is more descriptive of actual conditions of the land surface. Kessell<sup>76</sup> took a different tack, but espoused the same general philosophy in 1950. It was Kessell's student, Edwin H. Hammond, who produced the first visible effort along these lines in his 1951 dissertation on the Cape Region of Baja California.<sup>60</sup>

Hammond's thesis involves a verbal description of landform types, but is a definite precursor to quantitative description. From his study of Baja California, Hammond proceeded along more explicitly quantitative lines. In a paper given in 1954 he isolated the variables of interest to him: slope, composition, arrangement, and dimensions of the landscape. These were applied at the small scale of continental mapping first, using local relief, percentage of near level land, and profile (or distribution of near level land) to produce terrain types. Later, Hammond applied the same general techniques to more detailed mapping of Missouri and recognition of landform types there and in the northern Great Plains. Using the unit area approach, or a geometric grid of  $7\frac{1}{2}$  minute rectangles, he mapped local relief, the percentage of area in gentle slope and steep slope, the mean spacing of major crests, generalized profile types, and the dominant surface material to produce landform types for Missouri. This particular study is available only in an ONR Final Report, but a philosophical defense of the method as compared to explanatory description or genetic classification was published in 1962. Hammond's final effort in this thread of inquiry was a map of "Classes of Land-Surface Form" of the United States, which employed local relief, percentage of gentle slope, and profile type (or location of gentle slope), and is accompanied by an explanatory article. This map represents the only generic classification of landform for the United States as a whole at a scale which is sufficiently generalized to be comprehensible, and yet detailed enough

to be meaningful. The patterns displayed present a number of surprises to one accustomed to more conventional landform maps of various types, and this alone is sufficient reason to regard it as an important contribution to quantitative spatial geomorphology. Although the map was based chiefly on analysis of the 1:250,000 series topographic maps, more detailed future work will undoubtedly do little to alter the major lineaments presented on it.

Larger scale studies of smaller areas have more to offer besides refinement of the Hammond map of the United States, however. Two of Hammond's students offer examples. Both studies were published in 1963. Schmutde<sup>137</sup> examined the landforms of the lower Missouri River floodplain by means of verbal description, rather than quantitative, but recognized the significance of many features that escaped attention in most previous studies of alluvial plains. Zakrzewska<sup>184</sup> studied the landforms of the Upper Republican River basin in Kansas and adjoining states. Her work was much more quantitative in nature, utilizing a number of measures relating to relief, slope, and dissection of both the major and tributary valleys of the study area. More significant to the examination of this thread of inquiry, Zakrzewska proceeded beyond description to the verbal explanation of the patterns of roughness and local relief, asymmetry of valley cross sections, and valley parallelism that were revealed in her investigation.

One last study bears mention with the Hammond school of thought,

although the investigator was not, technically, Hammond's student. This is a classification of the landforms of Puerto Rico by Young,<sup>183</sup> who was at the University of Wisconsin at the same time as Hammond, and was influenced by him. The classification is a generic one, using relief and proportions of land in steep and gentle slope, as derived from 1:30,000 topographic maps. A three digit scheme is employed to define landform types, which are mapped. Verbal descriptions of the resulting landform regions make up much of the study.

Although related less directly to Hammond's influence, a number of other works produced at about this same point in time illustrate similar rationalizations concerning the quantitative description of the land surface. Salisbury<sup>133</sup> in 1957 produced a generic classification of landforms of Minnesota that owed a great deal to the influence of Hammond and to the ideas prevailing in geography at that time with respect to genetic landform classification. This classification used relief, slope, length of slope, materials, and drainage conditions to identify landform types. Various manipulations of the landform triangle to produce different indexes of topography were also involved. The typology involved a notation system similar to those employed in land use and land classification schemes (see Davis<sup>37</sup>). Correlations of the morphometric variables with each other and with the proportion of the land surface in different kinds of materials and drainage conditions were analyzed, but were not part of the typology. This aspect of the study was a forerunner of the empirical

explanations of landform elements that became popular a few years later. In other respects, the investigation is simply a more detailed exposition of some of the same principles that underly the Hammond and Young approaches, but applied to glaciated terrain, where both integrated and non-integrated surfaces were present. Measurements were performed within quadrats in an attempt to produce a universal system that could be used in both fluvially-dissected and non-fluvial terrains.

Some of the first attempts to merge quantitative description of land surface form with prediction were made by Walter F. Wood and his student, John C. Hook. Wood presented a paper on "The Relationships of Relief" in 1955 which created a storm of controversy because it invoked the then unfamiliar techniques of correlation and regression in order to develop means of predicting relief and slope.<sup>179</sup> Hook<sup>68</sup> utilized relief and a roughness index in order to better understand the relationships between topography and agricultural productivity in the northeastern United States. Wood pursued his ideas in a series of papers produced for the Environmental Protection Research Division of the Quartermaster Research and Engineering Command. These investigations dealt with other means of predicting land surface elements from each other, particularly useful where data are scanty, with line-of-sight, and culminated in a quantitative system for classifying landforms by the use of terrain factors.

Peltier,<sup>121</sup> in 1954, gave a similar paper in which he related average slope as computed by the Wentworth method to average local relief. The close relationship between slope and relief as established by Wood and Peltier served as the basis for additional studies several years later which

were designed to develop empirical relationships between landform elements, and to explain the location of patterns of certain elements by means of their spatial correspondence with patterns of other factors.

The first of these dealt with the relationships between slope and relief in glaciated terrain, an outgrowth of Salisbury's<sup>133</sup> generic landform classification of Minnesota. It was demonstrated that a high degree of relationship, and therefore of spatial explanation, exists in glaciated areas that have been affected by stream dissection, but that the relationship weakens in morainic topography. The jumble of slopes resulting from glacial deposition produces a non-integrated landscape with a disorderly arrangement, leading to low predictability. Salisbury also examined the three dimensions of the landform triangle, in addition to slope inclination, as they relate to lithology in the Mississippian Plateaus of Kentucky. Both stream-dissected and karst landscapes were involved. Lithologic variation proved to be important as a predictor of morphometry, but the topographic and stratigraphic position of the lithologic units modified their influence upon the parameters studied. These two investigations are virtually the only attempts to examine the spatial variation of landform elements on both fluvial and non-fluvial landscapes simultaneously.

LaVelle<sup>86</sup> examined certain aspects of karst morphometry, specifically karst depression elongation and orientation relative to structural trends in south-central Kentucky. He attempted to develop subterranean drainage



systems, and related the elongation and structural alignment of the depressions to their position within these systems, to the relief of the proposed basins and depressions, and to the character of the limestones in the area. McConnell<sup>97</sup> investigated mean topographic slope in a glaciated region near the Mississippi River, where the influence of developing stream systems could be strongly felt. He found that the vertical dimension of relief was the most important predictor of slope on the upland surfaces, but that channel slope, position, orientation, degree of dissection, and parent material also played a role. Roberts<sup>130</sup> examined a total of 29 variables, including morphometric factors and related erosional variables in a series of stream-dissected glaciated basins in the Middle West. The spatial variability of slopes in these drainage basins was considered in a series of models, which were then combined in a multiple regression framework. Six variables proved to be significant in the combinatorial model. These related to mean precipitation and the Wischmeier erosional index, to the perimeter of the basin, to density of first order streams, and to position within the basin with respect to its perimeter.

A strong trend may be recognized in this thread of inquiry with these latter studies. First came pure description of the landscape for its own sake and for objective comparison from place-to-place. This was followed by examination of the relationships between variables that measure land surface form, to provide empirical explanations of spatial variation. Increasingly, however, it was recognized that fluvial processes and slope

development could not be ignored in improving the prediction equations. Consequently, cross-sectional laws were being found to be less than satisfactory in providing acceptable understanding of surface form, and a means was sought whereby process laws could be derived more efficiently. Direct attention to the manner in which fluvial processes affect surface form appears to be the course this thread of inquiry is taking. This is the path taken, more or less, by the Columbia school of quantitative geomorphology, working with Strahler. The detour taken by many of the investigators cited above cannot be appreciated, however, without an appreciation of the attitude towards the study of landforms that was extant at the time.

The geological column in Table 1 (left hand side) reveals a more direct link to the influence of Horton. The application of principles of fluid mechanics and hydraulics to streams and to the landscapes created by stream action demands quantification of the various parameters. The geologists in the left hand column pursued this goal largely by providing more accurate descriptions of the land surface envelope, and of the interrelationships between the different variables that were involved. Most of the works in the early years in the left hand column were a part of the Columbia school. In the early 1950's Strahler<sup>154</sup> provided the most cogent arguments in favor of the introduction of statistical and mathematical techniques to the field. Although considerable opposition to the introduction of quantitative techniques was present in the early years of the quantitative revolution,

much of the comment was directed towards programmatic offerings as opposed to substantive efforts. That is to say, there was criticism to the effect that less space should be devoted to suggesting how research might be accomplished, and more effort directed towards providing examples that employed quantitative techniques. Actually, many of the studies of the Columbia group, even in the mid-fifties before quantification was well accepted, were directed towards examination of the variables involved in landscape denudation by fluvial agencies. Thus, Miller<sup>106</sup> examined the influence of structure (1953), Melton<sup>101</sup> that of climate (1957), Coates<sup>27</sup> viewed the role of lithology (1958), Schumm<sup>139</sup> slope processes (1954), and Morisawa<sup>110</sup> streamflow (1959).

A considerable amount of effort was devoted to refinement of morphometric techniques, particularly in the Columbia school. Chorley was especially active in examining the role of operator variance, or in developing more rapid and efficient means of describing the surface slope envelope. The process of refinement has continued to the present, as witness Schneider's<sup>138</sup> work on the accuracy of drainage densities derived from topographic maps (1961), Scheidegger's<sup>135</sup> concern with the effect of map scale on stream order (1966), Haan and Johnson's<sup>57</sup> concern with rapid determination of hypsometric curves (1966), Coffman's<sup>28</sup> work on parameter measurement (1970), and Pike and Wilson's<sup>123</sup> considerations of a number of hypsometric techniques (1971).

The examples from the left hand or "geological" column were almost exclusively drawn from fluvially-dissected landscapes. In large part this may be ascribed to the impact of Horton, but it also derives from the greater ease with which physical laws related to hydraulics can be applied to such landscapes. From the early days of application of quantitative methods to geomorphological problems, beginning with Horton or Strahler's 1950 study of the "Equilibrium theory of erosional slopes", the geological column was more concerned with the utilization of physical laws and mathematical structures than were the investigators who contributed to the right hand column. The right hand column was comprised mainly of geographers who were more concerned with providing pure descriptions of the land surface slope envelope. To a considerable degree the geologists of the left hand column were more concerned with development of precise data to be employed in studies of geomorphological processes that were replacing the pre-World War II concern with elaborate accounts of earth history.

The geographical thread as outlined in the right hand column did not derive directly from Horton, but rather found its roots in the dissatisfaction of geographers with genetic geomorphology. While it is possible to explain any spatial distribution by tracing its evolution back through time, this is an inefficient intellectual device for the development of laws and generalizations. Geographers needed data on the nature of the land surface form that could be employed in non-physical studies, as well as description of the land surface in objective,

comparable terms. It is not surprising that the major effort in the early years in particular was devoted to the development of "universal" parameters for describing land surface form, as illustrated by the work of Hammond, Young, and Salisbury. Parallel studies were underway in the military, Wood, and Snell's<sup>179</sup> 1960 classification scheme being but one example. Both single factor and multi-factorial quantifications of land surface form and related characteristics have been involved. Where several factors are considered, the usual plan has been to develop a classification scheme that would permit the delineation of landform regions. Just as quantification of single factors gives way to development of empirical generalizations involving several parameters, so the passage of time as revealed in Table 1 demonstrates increasing sophistication in landform classification; Lewis'<sup>88</sup> 1969 study on Indiana is an example.

The turning point in the geographical column occurs in the early 1960's. Although it is possible to find purely descriptive studies after this date (Schmudde's<sup>137</sup> 1963 study of the Lower Missouri floodplain is a case in point) or some attention to deduction from physical laws or mathematical relationships before, it is in the early sixties that the geographical and geological threads of inquiry that deal with description become increasingly intertwined. Moreover, after the early sixties it becomes increasingly difficult to place a study within the descriptive thread rather than in one dealing more directly with fluvial processes. Non-fluvial landscapes came in for a certain amount of attention during this interim period. Reed, Galvin, and Miller<sup>129</sup> discussed glacial

landforms in 1962, as did Salisbury,<sup>133</sup> and LaValle<sup>86</sup> published several papers on the karst of Kentucky in the mid-sixties. Many of the geographical studies of the period dealt with fluvial landscapes; however, although, compared with investigations originating in geology in the same time period, the geographical work was more descriptive or empirical.

By the late 1960's, there is little to distinguish whether a work originated in geology or geography, although the latter field continues to demonstrate a more obvious specific concern with spatial variation.

Fluvial Processes and Forms -- Geometry of Channels and Valleys --  
Stream Profile Thread of Inquiry

The application of quantitative methods to the study of streams goes back far beyond Horton's influence in American geomorphology. The work of G. K. Gilbert in the nineteenth century serves as an example. Streams, moreover, are studied by engineers and hydrologists as well as by earth scientists and in those disciplines there does not appear to have been any conscious "revolution" in which numbers were entered into the methodology despite the dismay of much of the profession. It is significant that Horton came from this area of knowledge and made his contribution to geomorphology at a critical point in its history. Increasing dissatisfaction with historical -- genetic studies and an increasing concern for the role of process or for more precise forms of description were coupled with a vastly increased amount of data in the post-World War II years. Much of this information dealt with streams and stream flow and the distinction into two threads of inquiry is made despite the high degree of interrelation-

ship. Many studies do deal exclusively with hydrologic explanations of stream flow without concern for what is occurring in the channels in terms of landform development. Others focus upon channel processes and their implications with little attention given to water beyond recognition of the fact that it is doing the work of modifying channel form.

The geometry of stream channels and their valleys involves consideration of the form of the stream profile as well as the cross-section of the channel. Fluvial processes create and change these forms and profiles, thus there is good reason for including all these elements within a single thread of inquiry despite the fact that certain investigations are quite specialized in their outlook. Most of the work in the early years of this thread of inquiry was accomplished by personnel associated with the U. S. Geological Survey (Table 2a). It has been suggested that much of the work derived directly from the vast amount of data that was accumulating on stream flow and load, and the attendant field investigations. This led to the development of the "hydraulic geometry" methodology of Leopold and Maddock<sup>87</sup> in 1953. Later studies are clear derivatives of this school of thought although many were apparently developed independently, at least in their early stages.

This thread of inquiry developed with little explicit attention given to the quantitative revolution. It was much more concerned with process than with description, although it relied heavily on empirical relationships between variables in the early years. Despite its insulation,

the thread is inextricably bound into the quantitative revolution and provided some of the best rationalizations for acceptance of the new methodology in geomorphology. There is a clear association of this thread with the development of ideas concerning equilibrium. In fact, the entire rejuvenation of a concept that had been more or less forgotten since the work of Gilbert depended largely upon the evidence provided by channel and slope processes. The attention to equilibrium also provides the clearest tie between these studies dealing with channels and valley bottoms and those that focus upon upland slopes.

As the "Description of land surface form" thread of inquiry turned more and more to a consideration of processes, most of the attention was diverted to the examination of channel and valley form and activity. This was a natural outgrowth of the Hortonian influence that was so strong in the early years of the quantitative revolution. Thus, the Columbia-Strahler school and many geographers became more and more explicitly involved with fluvial processes. They were joined in the 1960's by others who were trained during the quantitative revolution, or else gained an appreciation of the possible contributions to be made by the application of quantitative techniques.

Most of the early studies in the thread of inquiry dealt rather explicitly with questions concerning equilibrium states either in channels or in the landscape and thus directly supported a non-cyclic explanation of land surface development. The work of Hack<sup>58</sup> is particularly germane



in this regard, although equilibrium states are virtually implicit in any of the work on longitudinal profiles or hydraulic geometry, even that appearing in the 1950's. Greater attention was directed towards derivation from physical laws and less towards empirical relationships derived from laborious field investigations as the 1960's progressed. A considerable amount of effort was expended towards improvement of prediction equations in a variety of environmental situations. Thus, within the hydraulic geometry paradigm, Hack<sup>58</sup> examined glacial terrains (1965), Pestrong<sup>122</sup> estuaries (1965), Lewis<sup>88</sup> humid tropical basins (1966), Stephenson<sup>152</sup> the Southern Blue Ridge (1967) and Knox<sup>81</sup> the glaciated and "driftless" hills and plains of Iowa and Wisconsin (1970). Geographers in particular appear to be intrigued by the differences in relationships that are imposed by spatial variation in environments. Many of the studies challenged the developing concepts or offered modifications of some of the relationships (see especially Carlston<sup>18</sup>), while others devoted attention to discordances or disequilibrium conditions, particularly those arising from climatic change (Schumm<sup>139</sup>, 1965 and various papers by Dury<sup>41</sup> on misfit streams).

The application of physical laws concerning equilibrium and steady states leads quite close to a consideration of random processes or random effects on form or pattern. Thus, stochastic processes and topology become quite important in the late 1960's, sufficient to encourage recognition of a separate thread of inquiry dealing with the manner in which drainage nets are created or extended.

Development of Drainage Nets and Topology Thread of Inquiry

As a subset of the fluvial process and form thread it is worth noting those investigations that dealt rather specifically with the development and nature of drainage nets. Not all of these studies derived directly from Horton, but most owe him a considerable debt, at least in terms of the methodological break with tradition that his paper poses. The works cited in Table 2b are far removed in their focus from the considerations of drainage patterns as explicated in classical genetic geomorphology, and perpetuated to a considerable degree in textbooks. Actually, strange bedfellows appear on this list, it is only in an overview of the field that they appear to present a thread of inquiry.

Ruhe<sup>131</sup> examined the nature of drainage nets on drift topographies of varying ages in which stream process was more implicit and the goal was chiefly the differentiation of glacial stages and substages. Hack, by contrast, used glaciation in a different manner as presenting an initial surface that could be dated with some assurance and therefore permitted interpretations of how fluvial processes could create a drainage net. An analogous situation and opportunity is presented in gently-sloping coastlines where numerous studies were concentrated (Hack,<sup>50</sup> 1957, Ahnert,<sup>1</sup> 1960, Myrick and Leopold,<sup>112</sup> 1963, and Pestrong,<sup>122</sup> 1965). Some of the Columbia school were involved, although largely through examination of slope processes (Smith,<sup>148</sup> 1950, Schumn,<sup>139</sup> 1956), with the exception of Morisawa's<sup>116</sup> use of the unique opportunity

afforded by an earthquake to examine the development of a new drainage system (1964).

These investigations were related fairly closely to the work in channel geometry and the application of physical laws to streams and their workings. Another set can be recognized that focuses more upon the mathematical implications in the description of networks. The thread in late 60's and early 70's is given over almost entirely to topological considerations.

#### Explanation of Stream Flow Thread of Inquiry

The third subset dealing with fluvial process and form focuses upon the water in the channel rather than channel configurations or patterns and nature of development (Table 2c). Many more studies could be quoted from the field of hydrology than appear here but these give some flavor of the progress of the field, especially as geomorphologists have contributed to it. Changes in climate, differences in climate from place to place, and the complex interrelationships between the atmosphere and the lithosphere in their interface form the focus of attention of the various investigations. Trends in the nature of work conducted under this thread of inquiry are not so distinct as in other threads because the studies quoted here frequently tie to work underway in a variety of threads. Thus, Smith and Wischmeier<sup>146</sup> (1957) and Schumm and Lusby<sup>139</sup> (1963) relate to slope development, Brush<sup>15</sup> (1961), Salisbury, et. al.<sup>133</sup> (1968), and Knox<sup>91</sup> (1970) relate to channel geometry to cite several examples.

Systems of Landscape Explanation, Including Slope Development, Thread of Inquiry

Discussions of how landscapes are formed or modified inevitably evolve around questions of stream activity, if long term geologic time is the frame of reference. Shorter term frames of reference involve differences in kinds of processes, particularly as imposed by agents other than running water. Perhaps the reason systems of landscape explanation are fluvially-oriented, then, goes back to Davis and the Darwinian influence; however much an escape from this orbit is plotted. It is not surprising, then, that the landscape explanation thread of inquiry is also inexorably tied up with fluvial form and process. The purpose in distinguishing these studies as constituting, or at least contributing to a distinct thread of inquiry, lies in their attention to what is happening to the landscape in a broader sense, rather than simply to stream channels. It might be suggested that the "Systems of landscape explanation" thread provides both the initial rationale and the final tie-in or analysis of fluvial studies.

The early 1950's represent the break from classical historical-genetic studies in that Strahler and the Columbia school applied physical laws more explicitly to the fluvial landscape and to the development of the slopes of those surfaces (Table 3). Equilibrium, steady state, and systems analysis were added to the concepts that were involved in landscape interpretation.

Hack's enunciation and rejuvenation of the dynamic equilibrium

concept sparked considerable controversy from many who were opposed to either its methods or its conclusions (Bretz,<sup>11</sup> 1962, Holmes,<sup>67</sup> 1964). Although many of the studies both in this thread of inquiry and in the channel geometry thread utilize the concept of dynamic equilibrium, either as a basic assumption of conditions or to contribute to its refinement (McConnell,<sup>97</sup> 1964, Moultrie,<sup>111</sup> 1971), not all of the investigations that treat equilibrium have been totally supportive (Smalley and Vita-Finai,<sup>144</sup> 1969). Some studies have attempted to bridge the gap between equilibrium time-independent concepts and systems of landscape explanation and the classical time-dependent or cyclic systems of explanation (Schumm and Lichty,<sup>139</sup> 1965). Non-linear explanations that may not be time-dependent in the classical sense have gained favor in more recent years, due in large part to supportive evidence from climatology. One of the more consistent trains of thought in this vein is that espoused by Ruhe and his associates<sup>131</sup> (Ruhe, *et al.*, 1967, Ruhe, *et al.*, 1968, Ruhe and Walker, 1968, Walker and Ruhe, 1968). Most of the evidence of the Ruhe "school" has been pedologic or tied to development of soil landscapes and illustrates the importance of the three-dimensional viewpoint in the interpretation of landscape.

Throughout this entire thread of inquiry there appear publications which are most appropriately concerned with systems of landscape explanation that have been written by authors who at the same time are working in other areas, particularly fluvial geomorphology, and are contributing simultaneously to other threads of inquiry. This points up two facts, the close

interrelationship of this thread with others dealing with fluvial morphology -- whence cometh, after all, the principal "explanations" of landscape form in a general framework and the fact that there is no distinct trend visible in the systems of explanation thread. There is one exception to this conclusion and it has an importance that transcends most other controversies that have arisen in the past decade or so. This is the fact that in most recent years slope development studies in particular have embraced quantification, regardless of whether the slopes are thought to have formed by fluvial or non-fluvial processes of development. Arguments may rage over whether equilibrium is a viable concept to apply to this or that landscape, but support for those arguments are likely to be posed in statistical or mathematical frameworks. The use of numbers as evidence is now universal throughout geomorphology and equations are taken for granted rather than offered with a defensive rationalization. The quantitative revolution is over and the best evidence for this in geomorphology lies in the discussion of systems of landscape explanation -- slope development.

### Conclusions

Geographers entered the arena of quantitative geomorphology largely for the purpose of providing more precise information about the land surface slope envelope. Their early works were almost purely descriptive, usually involving single elements of the fundamental landform triangle, or considering combinations of elements in classification schemes. The next stage dealt more specifically with explanations of the spatial variation of landform

elements. Such explanations were highly empirical at first and were concerned chiefly with the spatial covariation of slope and relief for example. Empirical explanations proved to be rather sterile, however, and the role of process in at least the hypothesis formulation stage of an investigation was recognized -- at first implicitly and then explicitly. Fluvial processes were the first to be considered, although by the late 1960's quantification had entered into all branches of geomorphology.

By contrast with the geographers, the U. S. Geological Survey "school" was little concerned with description except on an incidental basis. Their principal interest from the outset lay in the examination of the processes taking place in streams and the forms that resulted from stream action. Quantification was intuitively necessary to such an undertaking, particularly when much of the impetus came from engineering, and there appears to have been no defensive attitude involved in their embrace of statistics and mathematics.

The Columbia school occupies a middle ground. The tone of many of the early works is clearly defensive in espousing the cause of quantification, particularly in the case of Strahler, who bore the brunt of the proselytizing. The description of surface form for its own sake never played as important a role as it did in geography and was quite explicitly rationalized from the point of view of examination of fluvial processes. The thread to Horton was explicit and direct.

By the early to mid-1960's the various innovation points or schools of thought had been inextricably intertwined and it is not profitable to recognize distinctions past that time. Threads of inquiry along certain foci of interest are somewhat recognizable, however, and offer efficiencies in considering the nature and progress of quantitative geomorphology. By the early 1970's the very phrase "quantitative geomorphology" is virtually a redundancy, so widespread and pervading are the use of such techniques in all areas of interest.



TABLE 1

DESCRIPTION OF LAND SURFACE FORM THREAD OF INQUIRY

1945	Horton	Erosional development of streams and drainage basins		
1947	Langbein	Topographic characteristics of drainage basins		
1950	Strahler	Equilibrium theory of erosional slope	Calef	Slope studies northern Illinois
	Smith	Standards grading texture of erosional topography	Kesseli	Geomorphic landscapes
	Ruhe	Graphic analysis drift topog.		
1951			Hammond	Geomorphic study Baja California
1952	Strahler	Dynamic basis geomorphology Hypsometric analysis erosional topography	Calef	Relative relief map of Illinois
	Chapman	New quantitative method topographic analysis Applications of SSO diagram	Thoman	Method of constructing average slope maps
1953	Miller	Quantitative geomorphic study drainage basin character	Calef & Newcomb	Average slope map of Illinois
1954	Strahler	Statistical analysis in geomorphic research	Young	Geographic classifi- cation landforms Puerto Rico
	Schumm	Quantitative slope analysis Evolution drainage systems & slopes, Perth Amboy	Hammond	Small scale continental landform maps Objective approach description terrain
			Peltier	Properties average topographic slope
1955			Wood	Relationships of relief
			Hook	Relationship roughness terrain and agric.
1957	Strahler	Quantitative analysis watershed geomorphology	Nir	Ratio relative & abso- lute altitudes Mt. Carmel
	Morisawa	Accuracy determination stream lengths from topo. maps	Wood & Snell	Dispersion geomorphic data around measures central tendency
	Chorley	Illustrating laws morphometry Climate and morphometry	Salisbury	Generic classification landforms Minnesota
	et al	New standard estimating drainage basin shape		
	Melton	Analysis relations among elements climate, surface properties, geomorphology		
1958	Strahler	Dimensional analysis applied fluviially eroded landforms	Hammond	Procedures descriptive analysis terrain
	Melton	List sample parameters quant. properties landforms	Snell	Device & method anal- yzing line of sight

TABLE 1 (continued)

		Correlation structure morpho- metric prop. drainage systems		
	Chorley	Group operator variance morpho- metric work with maps		
	Morisawa	Measurement drainage basin outline form		
	Chapman & Rioux	Statistical study topog., sheet- ing, jointing in granite		
	Coates	Quantitative geomorphology small drainage basins Indiana		
1959	Tanner	Examples departure from Gaussian geomorphic anal.	Van Lopik & Kolb	Handbook, technique desert terrain analog
	Chorley & Morley	Simplified approximation hypsometric interval	Hook	Quantification land- form characteristics
	Melton	Derivation Strahler's channel ordering system	Wood & Snell	Predictive methods topog. analysis, I & II
	Krumbein	Trend surface analysis contour-type maps	Wood & Snell	Prelim. invest. method predict line-of-sight
	Chorley	Geomorphic significance Oxford soils		
	Morisawa	Relation quant. geomorph. to stream flow Appalachian Plat. Relation morphometric prop. to runoff Little Mill Creek, O.		
1960	Strahler & Koons	Objective & quant. field methods terrain analysis	Miller & Summerson	Slope-zone maps
	Maxwell	Quan. geomorph. San Dimas forest, Calif.	Wood & Snell	Quantitative system classifying landforms
	Tanner	Numerical comparison geo- morphic samples		
	Scheidegger	Mathematical methods in geology		
1961	Yasso	Geometry spits & associated foreshore forms	Snell	Method predicting line-of-sight capabil.
	Schneider	Note on accuracy drainage densities from topo. maps		
	Scheidegger	Theoretical geomorphology.		
1962	Chorley & Morgan	Comparative geomorph. features Unaka Mts. & Dartmoor	Hammond	Land form geography & land form description
	Reed, Galvin, & Miller	Some aspects drumlin geometry	Woodruff & Evenden	Geomorphic measureme aerial photos
	Morisawa	Quantitative geomorph. watersheds Appal. Plat.	Peltier	Area sampling for terrain analysis
			Salisbury	Relief: slope relation glaciated terrain
1963	Troeh	Quantitative landform para- meters correlated soils NY	Roberts	Empirical regularities Humber R. net

TABLE 1 (continued)

	Carlston	Drainage density & stream flow	Hoy & Taylor Schmudde	Descriptive classif. of terrain Some aspects landforms lower Mo. R. floodplain
			Zakrzewska	Analysis landforms central Great Plains
1964	Strahler	Quantitative geomorphology drainage basins & channel networks	McConnell	Quant. aspects slope inclination upper Miss. valley
	Troeh	Landform parameters correlated to soil drainage		
	Scheidegger	Some implications statistical mechanics geomorph.		
	Labowe	Stream junction angles dendritic pattern		
1965	Vadnais	Quant. terrain factors related to soil parent materials	McConnell	Comments Fenneman & steepness subregional slope humid U.S.
	Troeh	Landform equations fitted to contour maps	Woodruff	Frequency dist. elevations as index terrain classification
	Stone & Dugundji	Study of microrelief: Mapping, classification, & quant. by Fourier analysis	LaValle	Areal variation karst topography
	Milling & Tuttle	Morphometric study two drainage basins	Salisbury	Landform-lithologic relationships Miss. Plat. of Ky.
	Lohnes	Quant. geomorph. drainage basins Iowa	Savigear	Technique morphological mapping
	Giusti & Schneider	Dist. branching river nets		
	Scheidegger	Algebra stream order numbers		
1966	Scheidegger	Effect map scale on stream order	Nunnally	Floodplain morphol. lower Ohio
	Scheidegger	Statistical desc. river network	Roberts	Spatial variation slopes & assoc. climatic & watershed variables
	Shreve	Statistical law stream numbers	King	Technique geomorph.
	Milton	Geomorphic irrelevance some drainage net laws	Clarke	Morphometric maps
	Haan & Johnson	Rapid determination hypsometric curves	McConnell	Statistical analysis spatial variability mean topog. slope stream dissected glacial matter.
	Merriam & Smith	Quantitative comparison contour maps		
	Curl	Caves as measure of karst		
	Chorley	Application statistical models to geomorphology		
1967	Smart	Comment on Horton's Law of stream numbers	LaValle	Some aspects linear karst depression devel.
	Hobson	Portran IV programs determine surface roughness	Hosking	Relations lithology surface form St. Francis

TABLE 1 (continued)

	Carson	Magnitude of variability in samples geomorph. charac. valley-side slopes	Woldenberg & Berry	Rivers & central places: Analogous sys.
	Morisawa	Relation discharge & stream length eastern U.S.		
	Smart & Surkan	Relation between mainstream length & area drainage basins		
1968	Lohnes & Joshi	Quantitative variations loess topography	Drummond & Dennis	Qualifying relief terms
	Thomas & Tuttle	Differentiation drift topog. statistical anal. slope	LaValle	Karst depression morph. SC Ky.
	Turner & Miles	Terrain analysis by computer		
	Smart	Mean stream numbers & branching ratios topol. random channel networks	Charlier	Quantitative analysis, geometrics, morpho.
	Smart	Statistical properties stream lengths	Dacey	Stream length & elev. model Leopold Langbe
	Scheidegger	Horton's law stream numbers		
	Scheidegger	Horton's law stream order number & temp.-analog in river nets		
	Scheidegger	Microcanonical ensembles of river nets		
	Scheidegger	Horton's laws stream lengths & drainage areas		
	Scheidegger	Random graph patterns drainage basins		
	Ranalli & Scheidegger	Test of topological structure river nets		
	Ranalli & Scheidegger	Topological significance stream labeling methods		
1969	Lustig	Quantitative analysis desert topography	Lewis	Analysis surficial landform properties-regions Ind. morphom
	Lustig	Trend surface analysis Basin & Range	McCoy	Drainage network anal K-band radar imagery
	Monmonier	Trends upland accordance Pa.		
	Liao & Scheidegger	Theoretical stream lengths & drainage areas in Horton's nets of various orders	Brown	Erosion & morphometry small drainage basins E. Puerto Rico
	Smart	Topological properties of channel networks	Dury	Relation morphometry runoff frequency
	James & Krumbein	Frequency distributions of stream link lengths	Woldenberg	Spatial order fluvial systems-Horton's law derived hexag. hierar
	Pollak	Numerical model Grand Canyon		
1970	Magorian	Vegetation & watershed shape	Graf	Geomorphology of glacial valley cross section
	Coffman	Parameter measurement in fluvial morphology		

TABLE 1 (continued)

	Koshinsky	Morphometry shield lakes in Saskatchewan	Folsom & Winters	Drainage orders in Michigan
	Hole	Drumlin morphology & soil relationships Wisc.		
1971	Pike & Wilson	Elevation-relief ratio, hypso- metric integral, & geomorphic area-altitude analysis	Graf	Quantitative analysis Pinedale landforms Beartooth Mts.

TABLE 2a

FLUVIAL PROCESSES AND FORMS: GEOMETRY OF CHANNELS  
AND VALLEYS, STREAM PROFILE THREAD OF INQUIRY

1945	Horton	Erosional development of streams and their drainage basins
1952	Holmes	Stream competence and the graded stream profile
1953	Leopold	Downstream change of velocity in rivers
	Leopold & Maddock	Hydraulic geometry of stream channels and some physiographic implications
	Land & Borland	River bed scour during floods
1954	Dury	Contributions to a general theory of meandering valleys
1955	Yatsu	On the longitudinal profile of the graded river
	Wolman	The natural channel of Brandywine Creek, Pa.
1956	Woodruff & Parizek	Influence of underlying rock structures on stream courses and valley profiles in the Georgia Piedmont
	Leopold & Miller	Ephemeral streams - hydraulic factors and their relation to the drainage net
	Sundborg	The River Klaralven, A study of fluvial processes
	Culling	Longitudinal profile of the Chiltern streams
1957	Wolman & Leopold	River flood plains: Some observations on their formation
	Leopold & Wolman	River channel patterns: Braided, meandering, and straight
	Hack	Studies of longitudinal stream profiles in Virginia & Maryland
	Culling	Multicyclic streams and the equilibrium theory of grade
	Culling	Equilibrium states in multicyclic streams & analysis of river-terrace profiles
	Gabriel et. al.	Influences of Coriolis force on river profiles
1958	Dury	Tests of a general theory of misfit streams
	Miller	High mountain streams - effects of geology on channel characteristics and bed material
	Melton	Correlation structure of morphometric properties of drainage systems and their controlling agents
1959	Wolman	Factors influencing erosion of a cohesive river bank
	Nixon	A study of bankfull discharge of rivers in England & Wales
	Dury	Analysis of regional flood frequency on the Neue & Great Ouse
	Hack & Young	Intrenched meanders of the North Fork Shenandoah River, Va.
	Broscoe	Quantitative analysis of longitudinal stream profiles of small watersheds

Table 2a (continued)

1960	Schumm	Effect of sediment type on shape & stratification modern fluvial deposits
	Schumm	The shape of alluvial channels in relation to sediment type
	Leopold & Wolman	River meanders
	Wolman & Miller	Magnitude and frequency of geomorphic processes
	Leopold & et. al.	Flow resistance insinuous or irregular channels
	Bragnold	Some aspects of the shape of river meanders
	Brice	Index for description of channel braiding
	Dury	Misfit streams: Problems in interpretation, discharge, dist.
	Fahnestock	Significance of a braided channel pattern
	Tanner	Helicoidal flow, a possible cause of meandering
	Ahnert	Estuarine meanders in the Chesapeake Bay area
1961	Fahnestock	Competence of a glacial stream
	Wolman & Brush	Factors controlling size & shape of alluvial channels in coarse noncohesive sands
	Schumm & Lichty	Recent flood-plain formation along the Cimarron R. in Kansas
	Schumm	Effect of sediment characteristics on erosion & deposition in ephemeral stream channels
	Schumm	Dimensions of some stable alluvial channels
	Hadley	Influence of riparian vegetation on channel shape, NE Arizona
	Brush	Drainage basins, channels, and flow characteristics of selected streams in C. Pa.
	Melton	Effect of sediment type on shape and stratification of some modern fluvial deposits
1962	Dury	Bankfull discharge: An example of its statistical relationships
	Dury	Results of seismic exploration of meandering valleys
	Wertz	Mechanism of erosion and deposition along channelways
	Leopold	Rivers
	Coleman	Observations of resistance coefficients in a natural channel
	Melton	Methods for measuring effect of environmental factors on channel properties
	Tinney	The process of channel degradation
	Tanner	Geomorphology and the sediment transport system
1963	Fahnestock	Morphology & hydrology of a glacial stream - White R., Wash.
	Schumm	Sinuosity of alluvial rivers on the Great Plains
	Schumm & Lichty	Channel widening & floodplain construction along Cimarron R., Kansas
	Myrick & Leopold	Hydraulic geometry of a small tidal estuary
	Carlston	Drainage density & stream flow
	Langbein	Theory for river channel adjustment

Table 2a (continued)

1964	Dury	General theory of meandering valleys
	Blench & Qurashi	Practical regime analysis of river slopes
	Langbein	Geometry of river channels
	Langbein & Leopold	Quasi-equilibrium states in channel morphology
	Langbein	Profiles of rivers of uniform discharge
	Foweraker	Quantitative studies in river sinuosity, with special reference to incised meanders of Ozark rivers
1965	Chang, et. al.	Total bed-material discharge in alluvial channels
	Emmett & Leopold	Downstream pattern of riverbed scour and fill
	Carlston	Relation of free meander geometry to stream discharge
	Hack	Postglacial drainage evolution & stream geometry Ontonagon, Michigan
	Cruff	Cross-channel transfer of linear momentum smooth rect. channels
1966	Maddock	Behavior of straight alluvial channels
	Benson & Thomas	A definition of dominant discharge
	Scheidegger & Langbein	Steady state in the stochastic theory of longitudinal river profile development
	Engelund	Hydraulic resistance of alluvial streams
	Simons & Richardson	Resistance to flow in natural channels
	Wertz	The flood cycle of ephemeral mountain streams in the SW U.S.
	Lewis	Adjustment of some hydrologic variables at discharge less 1 cfs
	Lewis	Relations of hydrology & geomorphology in a humid tropical basin
	Langbein & Leopold	River meanders: Theory of minimum variance
	Leopold & Langbein	River meanders
	Bagnold	An approach to the sediment transport problem from general physics
	Dury	Incised valley meanders on the Lower Colo River, New South Wales
1967	Schumm	Meander length of alluvial rivers
	Scheidegger	A thermodynamic analogy for meander systems
	Stephenson	Spatial variation of stream flow & related hydrologic characteristics, S. Blue Ridge Mts.
	Leopold & Skibitake	Observations on unmeasured rivers



Table 2a (continued)

1968	Thakur & Scheidegger	A test of the statistical theory of meander formation
	Dacey	The profile of a random stream
	Dacey	Stream length & elevation for model of Leopold & Langbein
	Noble & Palmquist	Meander growth in artificially straightened streams
	Dolling	Occurrence of pools and riffles: Element of quasi-equilibrium state of river channels
	Hooke	Steady-state relationships on arid-region alluvial fans
	Schumm	River adjustment to altered hydrologic regimen: Murrumbidgee R. and paleochannels, Australia
	Stahi & Fok	Hydraulic geometry of Illinois streams
	Stahl & Fok	Hydraulics & stream geometry
	Coffin	Relation channel width to vertical permeability streambed, Colo.
1969	Dury	Hydraulic geometry
	Leopold	The rapids and the pools -- Grand Canyon
	Beckinsale	River regimes
	Maddock	The behavior of straight open channels with movable beds
	Carlston	Downstream variations in hydraulic geometry of streams
	Carlston	Longitudinal slope characteristics rivers of midcontinent & Atlantic East Gulf slopes
	Salisbury, et. al.	The valleys of Iowa: Valley width-stream discharge relations
	Palmquist	"... In a valley proportionate to its size ..."
	Viletto	Channel morphology in N. W. Pa.
	Carey	Formation of floodplain lands
	Gilvarry	Geometric and physical scaling river dimensions earth & moon
1970	Tinkler	Pools, riffles, and meanders
	Knox	Stream channel adjustment to physiographic factors small basins
	Orme & Bailey	Effect of vegetation conversion & flood discharge on stream channel geometry: The case of S. California watersheds
	Hedman	Mean annual runoff as related to channel geometry, California
	Rango	Possible effects of precipitation modification on stream channel geometry & sediment yield
	Maddock	Indeterminate hydraulics of alluvial channels
	Thakur & Scheidegger	Chain model of river meanders
	Heede	Morphology of gullies in the Colorado Rocky Mts.
1971	McPherson	Downstream changes in sediment character in a high energy mountain stream channel

TABLE 2b

DEVELOPMENT OF DRAINAGE NETS AND TOPOLOGY  
THREAD OF INQUIRY

1945	Horton	Erosional development of streams and their drainage basins
1950	Smith	Standards for grading texture of erosional topography
1951	Ruhe	Graphic analysis of drift topographies
1952	Ruhe	Topographic discontinuities of the Des Moines Lobe
1956	Schumm	Evolution of drainage systems & slopes in badlands Perth Amboy
1957	Hack	Submerged river systems of Chesapeake Bay
1960	Ahnert	Estuarine meanders in the Chesapeake Bay area
1963	Myrick & Leopold Schenck	Hydraulic geometry of a small tidal estuary Simulation of evolution of drainage basin networks computer
1964	Morisawa	Development of drainage systems on an upraised lake floor
1965	Hack Pestrong	Postglacial drainage evolution & stream geometry, Ontonagon Development of drainage patterns on tidal marshes
1966	Scheidegger Shreve Woldenberg	Statistical description of river networks Statistical law of stream numbers Horton's laws justified in terms of allometric growth & steady state in open systems
1967	Scheidegger Scheidegger Shreve	Complete thermodynamic analogy for landscape evolution On the topology of river nets Infinite topologically random channel networks
1968	Anderson et al Liao & Scheidegger Ranalli & Scheidegger Ranalli & Scheidegger Scheidegger Scheidegger Scheidegger Smart Smart	Final report on evolution of drainage patterns Complete model for some branching-type phenomena in hydrology Topological significance of stream labelling methods A test of the topological structure of river nets Random graph patterns of drainage basins Microcanonical ensembles of river nets Horton's law of stream order number & a temperature-analog in river nets Statistical properties of stream lengths Mean stream numbers & branching ratios for topologically random channel networks

TABLE 2b ( Continued )

1969	Woldenberg	Spatial order in fluvial systems- Horton's laws derived from mixed hexagonal hierarchies of drainage basin areas
	Smart	Topological properties of channel networks
	Shreve	Stream lengths and basin areas in topologically random channel networks
1970	Smart	Use of topological information in processing data for channel networks
	Scheidegger	On the theory of evolution of river nets
	Werner	Horton's law of stream numbers for topologically random channel networks
	Wertz	The start of an ephemeral stream
	Howard et al	Topological and geometrical properties of braided streams
1971	Howard	Simulation of stream networks by headward growth & branching
	Howard	Simulation model of stream capture

TABLE 2c

EXPLANATION OF STREAM FLOW  
THREED OF INQUIRY

1945	Horton	Erosional development of streams & their drainage basins
1947	Langbein et al	Topographic characteristics of drainage basins
1957	Smith & Wischmeier	Factors affecting sheet and rill erosion
1958	Gumbel Melton  Langbein & Schumm	Statistical theory of floods and droughts Correlation structure of morphometric properties of drainage basins and their controlling agents Yield of sediment in relation to mean annual precipitation
1959	Morisawa  Morisawa  Dury Nixon	Relation of quantitative geomorphology to stream flow in representative watersheds of the Appalachian Plateaus Relation of morphometric properties to runoff in the Little Mill Creek, Ohio drainage basin Analysis of regional flood frequency on the Neue & Great Ouse Study of bankfull discharge of rivers in England & Wales
1960	Maxwell Wolman & Miller Hack & Goodlett	Quantitative geomorphology of San Dimas Experimental Forest, Ca Magnitude and frequency of geomorphic processes  Geomorphology & forest ecology of a mountain region C Appalach
1961	Hadley  Brush  Boyer Cooley & Akers	Some effects of microclimate on slope morphology & drainage basin development Drainage basins, channels, and flow characteristics of selected streams in C Pennsylvania Geomorphology & floods Late Cenozoic geohydrology in C & S parts Navajo & Apache Co, /
1962	Dury Benson Morisawa	Bankfull discharge: An example of its statistical relationship Factors influencing occurrence of floods in humid region Quantitative geomorphology of some watersheds in Appa. Plat.
1963	Schumm & Lusby Garde & Raju Fahnestock Carlston Wong Hely & Olmsted	Seasonal variation of infiltration capacity & runoff on hillslopes in western Colorado Regime criteria for alluvial streams Morphology & hydrology of a glacial stream- White R, Mt. Rainier Drainage density & stream flow Multivariate statistical model for predicting annual flood, N.E. Some relations between streamflow characteristics & the environment in the Delaware River region
1964	Benson Guy  Slayter & Mabbutt Langbein	Factors affecting occurrence of floods in the southwest Analysis of some storm-period variables affecting stream sediment transport Hydrology of arid & semiarid regions Hydrologic tools in paleoecological reconstruction

TABLE 2c ( continued )

1965	Sopper & Lull Schumm Shen Hidore Hidore	Streamflow characteristics of physiographic units in the NE Quaternary paleohydrology Use of analog models in the analysis of flood runoff Effects of glaciation on streamflow in the Upper Miss. Valley Landform characteristics affecting watershed yields on the Mississippi-Missouri interfluve
1966	Carlston Lull & Soper Roberts  Benson & Thomas Lewis	Effect of climate on drainage density & streamflow Factors that influence streamflow in the NE Spatial variation of slope & associated climatic and watershed variables A definition of dominant discharge Relations of hydrology & geomorphology in a humid tropical stream basin
1967	Morisawa Stephenson	Relation of discharge & stream length in eastern U.S. Spatial variation of stream flow & related hydrologic characteristics in selected basins of S Blue Ridge Mts.
1968	Carlston Salisbury et al	Slope-discharge relations for eight rivers in U.S. The valleys of Iowa: Valley width--discharge relationships
1969	Dury Williams & Fowler Beckinsale Schumm	Relation of morphometry to runoff frequency Preliminary report on an empirical analysis of drainage network adjustment to precipitation input River regimes Geomorphic implications of climatic changes
1970	Thomas & Benson Roberts & Klingeman Ulugür Knox  Hedman	Generalization of streamflow characteristics from drainage- basin characteristics Influence of landform and precipitation parameters on flood hydrographs Fluvial physiography as a factor in basin response Stream channel adjustment to physiographic factors in small drainage basins Mean annual runoff as related to channel geometry of selected streams in California
1971	Wong	Effect of stream size & land slope on recurrence of floods in New England

TABLE 3

SYSTEMS OF LANDSCAPE EXPLANATION, INCLUDING SLOPE DEVELOPMENT  
THREAD OF INQUIRY

1945	Horton	Erosional development of streams & their drainage basins
1950	Strahler	Davis' concepts of slope development viewed in the light of recent quantitative investigations
	Strahler	Equilibrium theory of erosional slopes approached by frequency distribution analysis
1952	Ruhe	Topographic discontinuities of the Des Moines Lobe
1953	Smith Miller	Erosional processes & landforms in Badlands Nat'l Mon., S.D. Quantitative geomorphic study of drainage basin characteristic in Clinch Mt. area, Va. & Tenn.
1954	Ruhe Schumm	Relations of properties of Wisconsin loess to topog. W. Iowa Evolution of drainage systems & slopes, Perth Amboy, N.J.
1956	Tanner Schumm Ruhe Wolman	Parallel slope retreat in humid climates The role of creep & rainwash on the retreat of badland slopes Geomorphic surfaces & the nature of soils Frequency & effective force in geomorphology
	Strahler	Quantitative slope analysis
	Ruhe & Scholtes	Ages & development of soil landscapes in relation to climatic & vegetational changes in Iowa
1957	Parizek & Woodruff	Description & origin of stone layers in soils of SE states
	King	Uniformitarian nature of hillslopes
	Smith & Wischmeier	Factors affecting sheet & rill erosion
	Schumm & Hadley	Arroyos & the semiarid cycle of erosion
	Culling	Equilibrium states in multicyclic streams & analysis of river-terrace profiles
	Melton	Analysis of relations among elements of climate, surface properties, and geomorphology
1958	Wischmeier & Smith	Rainfall energy & its relationship to soil loss
	Hack	Studies of longitudinal stream profiles in Va. & Md.
	Langbein & Schumm	Yield of sediment in relation to mean annual precipitation
	Coates	Quantitative geomorphology of small drainage basins, S. Ind.
1959	Chorley	Geomorphic significance of some Oxford soils
	Ruhe	Stone lines in soils
	Hack & Young	Intrenched meanders of North Fork of Shenandoah R., Va.
1960	Hack	Interpretation of erosional topography in humid temperate cli
	Hack & Goodlett	Geomorphology & forest ecology mountain region central Appal.
	Wolman & Miller	Magnitude & frequency of forces in geomorphic processes
	Melton	Intravalley variation in slope angles related to microclimate erosional development

TABLE 3 ( continued )

1960 (continued)	Scheidegger Culling	Analytical theory of slope development by undercutting Analytical theory of erosion
1961	Scheidegger Scheidegger Schumm & Hadley Menard Geyl	Mathematical models of slope development Evaluation of slope development theories Progress in the application of landform analysis in studies of semiarid erosion Some rates of regional erosion Morphometric analysis & the world-wide occurrence of stepped erosion surfaces
1962	Scheidegger Schumm Smith & Wischmaier Bretz Leopold & Langbein Chorley	Some modifications of the slope development problem Erosion on miniature pediments in Badlands Nat'l Monument, S.D. Rainfall erosion Dynamic equilibrium & the Ozark land forms Concept of entropy in landscape evolution .. Geomorphology & general systems theory
1963	Schumm & Lusby Everett Culling Kirby Zakrzewska Schumm Chorley	Seasonal variation of infiltration capacity & runoff on hillslopes in W. Colo. Slope movement, Neotoma Valley, S Ohio Soil creep & development of hillside slopes Study of rates of erosion & mass movements on slopes An analysis of landforms in a part of the central Great Plains Disparity between present rates of denudation & orogeny Diastrophic background to geomorphic thought
1964	Scheidegger Holmes Currey Schumm Douglas Chorley Langbein & Leopold	Lithologic variation in slope development theory Equilibrium in humid climate physiographic processes Preliminary study of valley asymmetry in Ogotoruk Creek, Alaska Seasonal variations of erosional rates & processes on hillslopes in western Colorado Intensity & periodicity in denudation processes with special reference to removal of material in solution by rivers Geography & analogue theory Quasi-equilibrium states in channel morphology
1965	Howard Culling Melton Schumm Melton Hack Schumm & Lichty	Geomorphological systems-- equilibrium & dynamics Theory of erosion on soil-covered slopes Debris-covered slopes of the southern Arizona desert-- consideration of their stability & sediment contribution Quaternary paleohydrology Geomorphic & paleoclimatic significance of alluvial deposits S Ariz. Geomorphology of Shenandoah Valley, Va & W Va. & origin of residual ore deposits Time, space, & causality in geomorphology
1966	Abnert Schumm & Chorley Schumm	Role of equilibrium concept in interpretation of landforms of fluvial erosion & deposition Talus, weathering, & scarp recession in Colo. Plateaus Development & evolution of hillslopes

TABLE 3 ( Continued )

1966 ( continued )		
	Scheidegger & Langbein	Probability concepts in geomorphology
	Leopold et al	Channel & hillslope processes in semiarid area New Mexico
	Hack	Interpretation of Cumberland escarpment & Highland Rim, SC Tenn. & NE Ala.
1967	Kennedy & Melton	Stream-valley asymmetry in an arctic-subarctic environment: conditions governing the geomorphic processes
	Schumm	Rates of surficial rock creep on hillslopes in W Colo.
	Kirkby	Measurement & theory of soil creep
	Ruhe et al	Landscape evolution & soil formation SW Iowa
	Scheidegger	A complete thermodynamic analogy for landscape evolution
	Hooke	Processes on arid-region alluvial fans
1968	Schumm	Speculations concerning paleohydrologic controls of terrestrial sedimentation
	Ruhe et al	Iowan drift problem, NE Iowa
	Ruhe & Walker	Hillslope models & soil formation- 1, Open systems
	Walker & Ruhe	Hillslope models & soil formation- 2, Closed systems
	Hooke	Steady-state relationships on arid-region alluvial fans in closed basins
1969	Meade	Errors in using modern stream-load data to estimate natural rates of denudation
	Kirkby	Erosion by water of hillslopes
	Carson	Models of hillslope development under mass failure
	Smalley & Vita-Finai	The concept of "system" in the earth sciences, partic. geomorph.
	Monmonier	Trends in upland accordance in Pennsylvania's Ridge & Valley
	Proctor	The North Bosque watershed- Inventory of a drainage basin
	Schumm	Geomorphic implications of climatic changes
	Ruhe	Quaternary landscapes of Iowa
1970	Ahnert	Functional relationships between denudation, relief, & uplift in large, mid-latitude drainage basins
	Emmett	The hydraulics of overland flow on hillslopes
	Welch	Substitution of space for time in a study of slope development
	Moultrie	Systems, computer simulation, & drainage basins
	Clark	Morphology of slope profiles-- A statistical method to critically analyze the slope hypotheses of Davis, Penck, King
	Cooke	Morphometric analysis of pediments & assoc. landforms, Mohave
	Kleiss	Hillslope sedimentation & soil formation in NE Iowa
	Gardner	Note on supply of material to debris slopes
	Branson & Owen	Plant cover, runoff, & sediment yield relationships, Mancos Sh.
	Benedict	Downslope soil movement in a Colo. alpine region: rates, processes, & climatic significance
1971	Moultrie	Computer simulation of erosional processes in a natural watershed: study of drainage basin development



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## CHAPTER IX

### CONCLUSIONS

This study of the achievements and of the impact of quantification in geography has shown the speed with which the discipline was transformed in a space of less than twenty years. After a short period of resistance and rejection in the 1950's, quantification became increasingly endorsed by the establishment and formal courses in quantification are now firmly entrenched in all but the most recalcitrant of graduate schools. As was shown in Chapter II, the quantitative revolution achieved far more than its initial purpose--in the minds of most; for many of the early proselytizers and practitioners of quantification clearly foresaw only dimly the path down which quantification would take them. Few apparently worked from the principles of mathematical induction and deduction discussed in this study in Chapter I; most rather adopted the trappings of quantification in the form of applying one of the more commonly used statistical descriptive or inferential techniques then widely used in those other disciplines with which geography had interacted. Thus the methodological revolution began as a shadow to the quantitative revolution but through time acquired substance as problems of interpretation, meaning, validation, and contribution of empirical research to theory construction increasingly arose. This interaction between technical developments and applications and the methodological foundations has more recently become intensively studied and Chapter III in this study elaborates on the issues involved and their relationships to further progress in quantification in the

discipline.

#### Current Status of Quantification

Quantification is no longer the unifying theme among quantitative researchers that it was as recently as five or so years ago. Given the quantitative expansion of quantitative research and the deep differences in viewpoints relating to fundamental methodological issues of research strategy that now exist, the status of quantitative work can only be evaluated with respect to the various quantitative "schools". While clearly these are not independent of one another it still remains true that each has its own level of achievement, special class of problems--both technical and methodological --and often its own criteria for evaluating its future progress. Mathematics is increasingly becoming the natural language of discourse although the practise of judging models by their fruits has led to the disturbing tendency for critical logic to become locked in some computerized algorithm and accessible only to those who have a sound knowledge of the particular language used.<sup>1</sup> It might be appropriate at this point to indicate some of these contrasting states of development in the form of contrasts between schools of quantitative geography that have ultimate objectives in common but that are distinguished by having chosen different approaches to their common goal.

Contrast, for example, the spatial forecasting models<sup>2</sup> of spatial structure with the approaches of conventional location theory (compare Ch. VII with Ch. VI). Researchers in the area of forecasting models have made great progress in constructing sensitive models relating aggregated

units of activities to one another with spatial specification and with many dynamic elements. They have become highly trained and competent in calibrating such models and in validating them against quite large geographic information systems. As Ch. VII indicates, they have more recently become involved in the interpretation problems attendant on their goal of evaluating alternative policy inputs. In contrast, conventional location theory has become more micro-behavior oriented and progress has been almost continuously stymied by discovery of the full complexity of human spatial decision-making and of the problems of constructing tractable analytic models which integrate the diverse findings of behavioral analyses in situations of complex initial environmental conditions, (see Ch. VI). Similarly, one might contrast the research in drainage-basin morphometry with that in the hydraulic characteristics of river networks, (Ch. VIII).

Contrast the level of quantitative development in the research of the spatial structuralist school<sup>3</sup> and in the Factorial Ecology School<sup>4</sup> with the still groping efforts at measuring basic elements of spatial behavior at the individual level. Compare, that is, Ch. IV with Ch. VI. The level of quantification of the structuralist school has been so much a part of graduate training in geography for the past decade that much current research can be regarded as "normal science" filling in the gaps of empirical knowledge and systematically measuring the values of critical parameters in various contexts. But within the school of analysers of individual spatial behavior is the contrast, according to ever more fundamental reductionist levels between those who adopt the structural approach (for example, the calibration of

distance-decay functions); those who wish to formally separate the environmental effects from the basic decision-model as, for example, in the scaling studies of perception and of space-preferences; and those who wish to further investigate the cognitive elements behind the decision-process.

Contrast the work in the regional area of those who saw quantitative regional geography as basically numerical taxonomy with those who, more recently, have asked that objective functions be defined to reach some accepted purpose (e.g. in political re-districting) or that the goal of numerical description of regional characteristics be more closely linked to theories of regional spatial structure (see Ch. V).

Other divergent directions in quantitative research can be identified. Much of the research in discovering order in spatial structure was predicated on the premise that from an identification of the underlying spatial order inferences could be made about the underlying process operating in the area. Recently this premise has been increasingly questioned and research has shown how different processes may yield similar spatial outcomes. The significance of such findings to the work of those whose goal is to construct control models that will yield desired elements of spatial form has been recognized. This "form-process" controversy will undoubtedly continue for some time for its ramifications on much that would now be regarded as "normal research" are great.<sup>5</sup>

The normative spatial research that Garrison in 1958 expected to quickly become a major thrust in economic geography<sup>6</sup> did not materialize then but even a cursory review of the contents of the journal Geographical Analysis

indicates a resurgence of operations-research-type models where, once again, the question of optimal spatial arrangements becomes paramount. With such questions, appropriate mathematical approaches are quite different from those used in other schools of quantitative geography and the degree of expertise by geographers generally in this area is quite weak. (Ch. VI indicated some notable exceptions to this statement).

One school of quantitative geography is developing around the spatial problems found outside the western context. Here interest in knowing the robustness of models developed in a western context for application to countries outside the western world--particularly in the developing countries--is central. This discovery of new contexts in which parameters which formerly had been thought to be independent of context are shown to assume values outside the domain of previous experience has challenged theorist and empiricist alike.<sup>7</sup>

In research in all of these "schools" the availability of quality data commensurate with the type of operations envisioned in the models that have been developed is questionable. Garrison's observation in 1958 that the 1960's would see large efforts made to construct such geographical information systems proved true<sup>8</sup> but the observation is likely to be equally true of the 1970's.

#### Achievements of Quantification

Many of the achievements of quantification in geography were implicit in the review above of the current status. However, to anyone familiar with the lethargy in geographic research twenty years ago, the overwhelming

achievement will be the egocentric sense of pride and fulfillment that the present status of the discipline as a fullfledged science operating within the mainstream of scientific thought and keyed in to the problems of contemporary society can be laid at the door of quantification. Geography has now joined the mainstream of both social and physical science. If the substantive achievements to date do not appear to match the considerable efforts involved, one has to appreciate the overhead-cost nature in terms of basic training, and re-orientation that has been involved. In other words, to date the most important achievement must be the new orientation of the geographic profession presenting as it does today, a resource ripe for further application to the problems of society.<sup>9</sup>

A second achievement is the progress that has been made (see Ch. IV) in response to Schaefer's call that geography should apply the methods of science to discover the morphological laws concerning the distribution of phenomena on the earth. The fact that such spatial-structural studies no longer have the research following they had in the early sixties should not obscure or diminish the importance of the fact that many such laws and regularities have been discovered and in many cases are now being used on a day to day basis in many practical applications. Though it may be fashionable to dismiss as pedestrian the efforts of those who have so laboriously calibrated many geographic models (e.g. the distance-decay function or the factorial ecology of American cities), such normal scientific effort has resulted in substantive findings which, though scattered in a plethora of publications and fugitive manuscripts, do nevertheless satisfy

Schaefer's criteria of a body of particularly spatial (geographic) laws.

Increasingly, the researcher today takes these "solutions" and calls them his "problems". These become problems in the sense of the need to identify more fundamental processes to account for them and in the sense also that society finds many of the patterns that are discovered to be undesirable in some social or overall-efficiency sense. Thus the efforts to construct control models, sensitivity tests and other theoretical enquiries to determine what may be changed in order for spatial outcomes to correspond closer to that desired. Contemporary quantitative geography is clearly entering the realm of the policy sciences in which desired ends are the "solutions" and where evaluating the effects of alternative strategies for control through sensitive models of the region of interest is the essential step along the path to the goal of formulating alternative means of reaching the policy-makers goals.

A third achievement is the methodological success experienced in building deductive theoretical systems from a set of basic postulates. Such experience in deducing spatial order from basic principles is especially useful now that we have reason to question many of the inferences about behavior made from observations on spatial form.<sup>10</sup> The fact that interpretations concerning causal mechanisms made from analyses of spatial form are ambiguous has led more researchers to become interested in causal model-building from ever more basic premises. Thus experiences in the modelling of theoretical systems<sup>11</sup> will increasingly find practical applications in working with applied problems.



A fourth achievement of the quantitative thrust in geography has been the raising of the general level of technical training in the profession. Fruitful contact with other disciplines in the past was impeded by the low level of technical expertise in the discipline. Chapter II indicates some of the factors that facilitated this trend in the early years of quantification. Since then, changes in university curriculums; changes in the image of the discipline by prospective students; and changes in the admission requirements of some graduate departments of geography have further accelerated this trend. Some members of the discipline have held essentially technical appointments in various universities.

A further achievement has been the changed structure of the discipline itself which quantification wrought. Earlier emphases on regional specialization gave way to an acceleration of systematically oriented specializations in the discipline. Perhaps more importantly--since this trend was present before quantification became a major issue--is the fact that quite disparate systematic areas of the discipline found common problems and therefore found the need to communicate when quantification was adopted as an integrated facet of the research approach. Students of spatial statistics, for example, irrespective of their systematic affiliation within the discipline, found themselves studying and discussing research in plant ecology, in central-place theory, or in Karst geomorphology. That the models being used were mathematical facilitated understanding where the subject matter was unfamiliar and further encouraged communication between branches of geography that had been essentially independent in earlier decades. Of course, the primary

emphasis on analyses of spatial form in the early years of the quantitative effort facilitated this interchange of ideas and of solutions.<sup>12</sup>

The resolution of many of the epistemological issues extant in the early years of quantification was an achievement that left undiluted the efforts that led to the substantive achievements of the last decade. A summary of the viewpoint that prevailed about 1968 was provided by Harvey<sup>13</sup> and also by Adams, Abler and Gould.<sup>14</sup> The fact that no reviews appeared that were critical of the position Harvey adopted there that the mode of explanation of the logical empiricists had been the mode for quantitative geography--a mode that Harvey advocated for the future at that time. The period of unanimity among quantitative geographers on this point is now over. Some of the recent viewpoints were discussed in Chapter III.

#### Future Developments

##### Technical

Technical developments in quantification appear to be easier to forecast since so many of the prospective developments are logical extrapolations of well-established trends. Always, of course, the innovative breakthroughs are in principle unpredictable. Speculation, however, might point to the expectation of novel developments from the combination of the increasingly self-critical posture of quantitative geography; the higher level of technical expertise; the greater involvement in applied problems; and the new positions on epistemology--the combination that results from integration of the achievements of quantification discussed earlier.

Within the technical area though, the further development of integrated geographic information systems seems certain. Integration will be the key facet of this development. The variety of input sources will increase as the technology and software in remote sensing develop; the orientation of census material to final use in such an information system will increasingly modify the practises of Census agencies and far more commonly will day-to-day data of governmental administrative bodies be incorporated in the systems developed. The relation between various geographic coordinate systems will be further explored and the capacity for checking, editing and updating such systems will improve.

Further developments in the design and efficiency of computer algorithms are to be expected--many of these will be heuristic to fill the gap between the more commonly recognized problems and the present intractable nature of the mathematical systems for deriving analytic solutions. In addition, there will be developments of integrated computer software systems that will be the analytical counterpart of the geographic data banks. Future developments in perfecting remote access capabilities to both data and programs from diverse locations will further speed the adoption of quantitative approaches to the solution of many variable, many area-type problems.

Although these developments could take place without any new types of hardware systems than presently on the market, it seems clear that technical developments by the computer science industries in this area will continue. Particularly in the realm of graphic input/output, expected developments have far-reaching implications in the conduct of geographic research.

### Substantive Developments

These are more difficult to forecast than technical developments. Certain trends are apparent in research currently underway and certain of the trends in curricula change should have predictable consequences. On both these counts it seems certain that the involvement of geographers in applied problems will increase as technical developments and training advances increases the capability of many researchers to handle complex problems in a formal way. Regional forecasting models for a wide range of socio-economic activities and indices will become as common as weather predictions --and no doubt will be as prone to error. But the concern to know the future state of environments so that public and private investment and land use control policies may be complementary will inexorably lead to a demand for high-quality projections in this area. Developments in geographic information systems will become connected to specific data banks covering most of the metropolitan regions. In the developing countries where spatial integration is not so much something to be discovered in the factor scores of some large matrix of flows as something to be achieved by rational spatial planning in the context of very scarce resources, forecasting models and planning models will be inextricably linked. Everywhere, the future pattern of spatial activities will be seen as something to be achieved rather than something to be predicted from the operation of inanimate forces outside of our control. As Harvey so aptly stated it, the solutions of conventional spatial analyses will increasingly come to be regarded as the problems to be solved. 15

This trend to forming the future rather than simply describing it will undoubtedly lead to a larger emphasis on normative, operations-research type work. To this point, most research in geography has either dealt with hypothetical problems or with small and very partial-type problems (as in industrial location studies). As the field of mathematical programming develops, the future of geographic research should increasingly incorporate the techniques and style of Operations-Research.

The trend toward quantitative behavioral geography will continue with the development of quite different research styles as geographers become more familiar at a deeper level of the parallel differences in approaches within the field of psychology; from which for some time to come the bulk of the inspiration for work in this area will derive. Certainly there will be a contrast between those researchers who attempt to uncover preferences from overt behavior; those who attempt to recover perceptions of environments and others who work at an even deeper cognitive level. There will be an increasing concern to operationalize decision-models at both the group level and the individual decision-making level. Basic measurement problems relating to all these issues will necessarily consume an inordinate proportion of the total research effort.

Research in the behavioral area and in the area of normative model is likely to become fused in a new research area concerned with the abilities of human intuition to solve location problems. Even if the techniques of mathematical programming were widely adopted for the solution of locational problems it is still likely that the vast proportion of location decisions will

be made at an intuitive level. Research to discover how well different kinds of people can make these decisions and research into the problem of training people to increase their skills in this area will develop.

The integration of findings in all the above research areas will be attempted by the location theorists of tomorrow for, in this (functionally speaking) ever smaller world, the desire to rationally organize this resource which, practically speaking, is virtually non-expandable in quantity--physical space--will become a paramount problem in the minds of many.

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INFLUENCE OF QUANTITATIVE METHODS ON THE  
UTILIZATION OF COMPUTERS IN GEOGRAPHIC RESEARCH

Geography has often been characterized as being concerned with the identification of the phenomena that give distinctive character to different areas.<sup>1</sup> Any phenomenon that is irregularly distributed over the earth's surface has the potential of filling this descriptive use. Phenomena themselves are associated in particular places, but association does not necessarily imply a relationship between them. Quite different and unrelated processes may be operating. Thus, geographers have attempted to describe the operation of specific processes wherever they might be located.

The map became the fundamental instrument of geographic research, providing the means to record the diverse phenomena found in an area. Map patterns are generated by plotting points, lines, and areas on a specified projection. This methodology has formalized as cartographic analysis.

The association of phenomena by map comparison usually was accomplished by visual observation. Later, a group of researchers at the University of Iowa demonstrated the inherent difficulties of visual map comparison.<sup>2</sup>

Regardless of such criticism, the point made here is quite different. Consider the nature of the discipline and its relation to others. A classification according to the degree to which theoretical procedures or explanations are used as contrasted with correlational procedures or explanations has been suggested.<sup>3</sup> The difference is based on the degree to which a discipline is characterized by statements describing the degree of relationship among more or less directly observable phenomena and one that is capable of deriving,

accounting for, or explaining relationships from principles that are not obvious (i. e., beyond observational knowledge). It is apparent, but noteworthy, that all sciences begin as largely correlational and progress toward the theoretical. A quote from The Science of Geography is relevant:

Geographers have long believed that correlations of spatial distributions are among the most ready keys to understanding existing or developing life systems, social systems, or environmental changes. As geographers undertook such studies in the past they favored heavily the empirical-inductive method. More recently, particularly since the end of the last war, theoretical-deductive methods have been applied.<sup>4</sup>

A few years earlier Burton wrote:

In the past decade geography has undergone a radical transformation of spirit and purpose, best described as the 'quantitative revolution.' The consequences of the revolution have yet to be worked out and are likely to involve the 'mathematization' of much of our discipline, with an attendant emphasis on the construction and testing of theoretical models.<sup>5</sup>

The quantitative revolution in geographic research did not take place without influences from several sources. Contributions of a substantive and methodological nature have been discussed elsewhere.<sup>6</sup> The command of information made possible by the computer has permitted the analyses of large amounts of data and this has been an important contributor to research productivity. Computers enable geographers to examine complex relationships which take place within and over space. The technological ability to handle data has grown much more rapidly than the ability to collect it. Thus, from a state of affairs where correlational studies were frequently based on obvious associations, simply because of the great arithmetical drudgery involved, geographic research has progressed to, and beyond, the stage in

which "every" available variable is correlated with every other one, in a "seek and ye shall find" approach. Clearly, high correlations do not make much sense unless the relationships they portray can be related to a developing body of theory. Currently, hypothesis-testing is seen to be more fruitful when the relationships being tested are generated from a formal treatment of the theoretical constructs. Quantitative methods provides a useful way of expressing theoretical arguments, and resulting hypotheses are explicitly and unambiguously stated. Now the problem becomes one of collecting data on the variables (or surrogates) that formal theory indicates should be related.

The number-crunching power of modern computers is only as useful as our theoretical developments permit. An expanding research productivity depends upon theory development, and theory, at some stage, must be tested. As Burton has pointed out:

The development and testing of theory is the only way to obtain new and verifiable knowledge and new and verifiable understandings.<sup>7</sup>

Testing need no longer be a correlational procedure, in the sense of observable features, but may involve decision-making models, computer simulation techniques, and the recovery of metric, from non-metric, information.<sup>8</sup>

Clearly, the research potential is inextricably bound up with the development of sound theory. Also, more diversity in the use of the "information machine" is a complementary enterprise.<sup>9</sup>

The development of testable models from theory presupposes that the data is available by which this can be accomplished. When explicit

statements of "process" are part of theory, data are usually required with a temporal dimension, as well as spatial. Likewise, the ability to derive a composite index from observable variables efficiently and objectively is necessary. For data collected from diverse sources, compatibility is unlikely, thereby stifling the activities of theory testing. The collection and presentation of observable data seems like an end in itself when information processing capabilities are not available.

Geographic research potential and the expansion of viable theory is directly related to the degree to which the practitioners of spatial analyses can harness the portentous generality of the modern computer-system. While any individual who cares to acquire the expertise can utilize a handy computer, results would be minimal if each researcher had to "go it alone." Fortunately, many applications are similar, at the level of computer circuitry, and much computer related equipment and software is easily adopted to the geographer's needs. In one sense, the geographer's data requirements are unique. In nearly every instance the data for geographic research must be spatially related. The simplest case would be the attachment of latitude and longitude to a bit of information about a point on the earth's surface. Data might also be assigned to areas which are defined by their spatial relationships to each other. At a higher level of abstraction there exist derived variables of spatial relevance, say, space preferences.<sup>10</sup> At this derived level the attachment to a particular point on the surface of the earth may be generalized. Nevertheless, the original data must have been collected in a spatial context.

A theory-laden environment for geographers must be responsive to the unique data needs of spatial analyses. Optimally a geographic information system should be available which could supply, on demand, temporally and spatially organized data relevant to the substantive needs of the researchers. Clearly, the information to be processed by the system should be specified by theory-conscious practitioners of spatial research.

The structure of an information system to serve the needs of the geographic theorists differs little from that which would meet the needs of many others. However, there must be an explicit attempt to supply a locationally meaningful dimension to all, or nearly all, of the information passing through the system.

Methods of referencing spatial location for use in an information system refers to the techniques of specifying location in a machine-readable form, often termed geocoding. Essentially, there are three alternative methods for identifying location: direct digitizing, area unit codes, and unique addressing, such as street addresses (or miles from stream mouth) which then may be translated to geographic coordinates or areal unit codes. Direct digitizing of observations to x-y coordinates is the usual method chosen when the data source consists of imagery, maps or other graphics. Assignment of areal unit codes to observations is a usual procedure when conducting field surveys. Depending on the type of survey and the sample size, areal units will vary considerably. Finally, a procedure which is gaining greater acceptance in urban areas is the machine translation of street addresses to either geographic coordinates or to areal unit codes. The street address translation process

requires a Geographic Base file, i. e., a directory of address ranges assigned to an areal unit for comparison with the input address requiring translation. The street address translation process obviously allows greater utilization of administrative record keeping files for use in analyses of urban spatial structure and transportation geography. Geocoding is only a small aspect of a total geographic information system, and while its importance to research efforts and theory development is significant, it would be unwise to gloss over other important aspects. It is useful, therefore, to briefly discuss the nature of an information system.

An information system is composed of basic elements including:

1. People
2. Computer and related packages
3. Data definition
4. Institutional procedures
5. Data collection methodology

These basic elements are related and must operate within prescribed system patterns that will:

1. meet operational requirements,
2. facilitate summarizing and analytical techniques,
3. assist in the search for program goals,
4. assist in evaluation and control,
5. aid information exchange.

The basic elements and the prescribed system patterns are more fully developed in Table 1 and the schematic diagram which follow. A careful examination of both displays indicates the need for careful consideration of each operational decision with respect to the influence on the geographical relevance of the

Table 1. Information System Elements

1. People

Set policy and establish programs

Develop and maintain information system hardware and software

Provide input data

Utilize the system to answer a broad range of questions

2. Computer and Related Packages

Hardware:

Computer  
Tape and disk drives  
Graphic display  
Remote input

Software:

Control system  
Analytic program packages  
Statistical routines  
Query procedures  
File manipulation

3. Data Definition

Descriptive:

Data items  
Indices and surrogates  
Sensitivity analyses and relationships  
Areal compatibility  
Temporal requirements

Contextual:

Norms  
Standards  
Assumptions

4. Institutional Procedures

Level of existing technology  
Functional/operational responsibility

5. Data Collection Methodology

Field surveys  
Automatic remote sensors  
Geocoding

resulting data. For example, graphic display for output and digitizing capabilities for input purposes would be of high priority in a geographic information system. However, in most computer installations these features are given short shrift. Thus, it is not unreasonable to suppose that some persistence will be needed to assure more than the minimal requirements of these "exotic" items in supportive systems to geographic model building and theory testing.

Regardless of the specifics, the basic principles of an information system are indicated in the diagram, which emphasizes a system development and a system utilization stage. The former stage is based primarily on the specification of data and the development of data handling techniques, including geocoding, transformations, and storage. System utilization requires that the information be addressable on files which can be queried, restructured, integrated, and easily and quickly updated. Also, norms and standards, established by the appropriate decision-makers, must easily be merged with these files. The users, in turn, are responsible for maintaining the flow of data into the system, thus assuring its relevance.

Clearly, the distinction between development and utilization has a special meaning with respect to the process. Development means the process of getting set up to continuously collect, process, and provide data files which meet the needs of the users, as well as the updating of procedures and equipment to accomplish this task. Utilization, on the other hand, means the processing of queries by interested users.



While the basic elements will differ in degree from one information system to another, the prescribed system patterns may, in fact, differ in more basic structural ways. More simply, the elements can be operationalized in different ways while serving the same purpose. Alternative structures can be evaluated with respect to how well each meets certain fundamental requirements.

Requirements depend primarily upon the needs of those supporting the operation in terms of use, but are constrained by the technological and monetary context. The phenomena of interest influence some of the above factors. For example, an air pollution control information system would require continuous information input requiring very sophisticated monitoring equipment, while a public facilities planning information system would require only population counts and characteristics for areal subunits for, say, yearly or bi-yearly intervals.

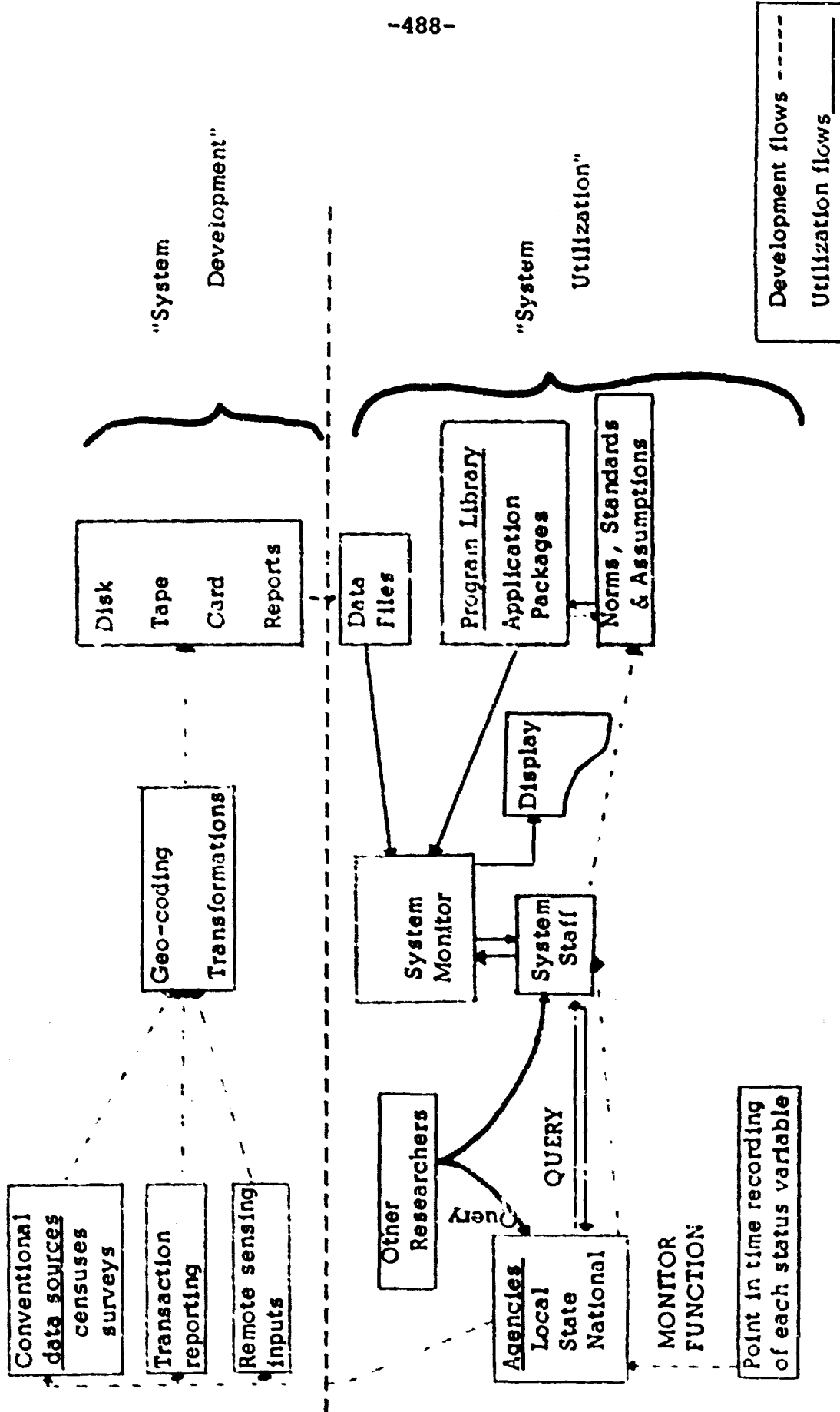
### Summary

The marriage of geography and quantitative methods has been fostered by the rapid development of computers, so that the current status of much disciplinary research depends on this technology. The desire to process huge data sets will bring greater utilization and increasing operational dependency on computing systems. Although this dependency is well recognized by many researchers, few have had any impact toward the acquisition of the equipment to be utilized and fewer still have had any impact on the development of the hardware.

STORAGE

COMPILATION

DATA



STATUS VARIABLES & CHANGE DETECTION      HARDWARE & STAFF      SOFTWARE & PROCEDURES

Schematic Representation: The Structure of an Information System

Careful consideration should be given to the requirements of an information system which would serve the needs of the geographic researcher. The process itself might prove enlightening! For, one thing the quantitative revolution has done to geographers is to make them data hungry, so that data being used is not always equivalent to that which would be desired. The result is that writing and testing theory is a roundabout procedure that could be circumvented with a purposefully developed geographic information system. Theoretically-relevant research productivity would expand accordingly. Clearly, the forefront of geography's theoreticians are beyond the correlational stage and are increasingly frustrated in the development of viable theory by the lack of data appropriate for their evaluation and validation.

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**Unclassified**

Security Classification

**DOCUMENT CONTROL DATA - R & D**

(Security classification of title, body of abstract and indexing annotation must be entered when the overall report is classified)

1. ORIGINATING ACTIVITY (Corporate author) <b>University of Iowa Iowa City, Iowa 52240</b>		2a. REPORT SECURITY CLASSIFICATION <b>Unclassified</b>	
		2b. GROUP	
3. REPORT TITLE  <b>QUANTITATIVE GEOGRAPHY: ACHIEVEMENTS AND PROSPECTS</b>			
4. DESCRIPTIVE NOTES (Type of report and inclusive dates) <b>Final Report</b>			
5. AUTHOR(S) (First name, middle initial, last name) <b>Gerard Rushton, Frank E. Horton, James B. Lindberg, Michael L. McNulty, David R. Reynolds, Neil E. Salisbury, Pete Taylor.</b>			
6. REPORT DATE <b>December 1971</b>		7a. TOTAL NO OF PAGES <b>490</b>	7b. NO OF REFS <b>879</b>
8a. CONTRACT OR GRANT NO <b>DAAK02-70-C-0185</b>		8b. ORIGINATOR'S REPORT NUMBER(S)	
A. PROJECT NO <b>4A061102B52D</b>			
		9a. OTHER REPORT NUMBERS (Any other numbers that may be assigned this report)	
10. DISTRIBUTION STATEMENT  <b>Unlimited distribution</b>			
11. SUPPLEMENTARY NOTES		12. SPONSORING/MONITORING AGENCY NAME(S) AND ADDRESS(ES)  <b>U.S. Army Engineer Topographic Laboratories Research Institute.</b>	
13. ABSTRACT  <b>The research goals and methodological approach of quantitative geography in its formative period (1955-1965) are described and some principles concerning the logic of scientific discovery which led to further adoption and modification of quantitative approaches in the discipline are discussed. Substantive achievements of quantification are described in relation to the analysis of spatial structure; mathematical regionalization; development of location theory from behavioral foundations; measurement problems in the analysis of spatial behavior; the development of spatial prediction models and in relation to quantitative spatial geomorphology. Forecasts of future developments in the area of quantitative geography are made and the adaptability of substantive findings for incorporation in an automated environment of data analysis are discussed.</b>			

DD FORM 1473

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Security Classification

14.	KEY WORDS	LINK A		LINK B		LINK C	
		ROLE	WT	ROLE	WT	ROLE	WT
	applied geography epistemology geographic methodology geographic theory geographic thought geography models modern geography physical geography quantitative geography quantitative techniques scientific methodology spatial analysis spatial statistics techniques of geographic analysis						

Unclassified

Security Classification